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TABLE OF CONTENTS.

OBSERVATIONS ON TRICHINA SPIRALIS (J. C. DALTON, M.D.)—EXPERIMENTS ON INSENSIBLE PERSPIRATION (JOHN C. DRAPER, M D.)—INDUCTION OF PREMATURE LABOR (GEO. T. ELLIOT, M.D.)—OSTEO-MYELITIS (JOHN A. LIDELL, A.M., M.D.)—ETIOLOGY AND PROPHYLAXIS OF BRIGHT'S DISEASE (GOUV. M. SMITH, M.D.)—PATHOLOGICAL CONDITIONS OF STERILITY IN THE FEMALE, (JAMES KAMMERER, M.D.)—PHYSIOLOGY AND PATHOLOGY OF SYMPATHETIC OR GANGLIONIC NERVOUS SYSTEM, O'REILLY PRIZE ESSAY (ROB'T T. EDES, M.D.)—CLIN. OBS. ON MALIGNANT DISEASES OF UTERUS (F. BARKER, M.D.)—SURGICAL THERAPEUTICS OF AIR PASSAGES (G. BUCK, M.D.)

INDEX.

N. B.—In consequence of an error in pagination, the number affixed to each heading is that of the page of the Paper itself, each of which is paged separately.

A.

Aphorisms of Sanctorio on Perspiration, Sensible and Insensible (Draper), p. 26.
Air Passages, A contribution to the Surgical Therapeutics of (G. Buck), p. 1 of Essay.
Arsenic, in Schirrhus Uteri (F. Barker), p. 20.

B.

Bright's DEisease, tiology of, etc., by Gouv. M. Smith, p. 1.
Buck, G. Contribution to the Surgical Therapeutics of the Air Passages, p. 1.
Barker, F. Some Clinical Obs. on the Malignant Diseases of the Uterus, p. 1.

C.

Complications which may arise in the operation for Ovariotomy (Peaslee), p. 86.
Conclusions as to Ovariotomy, " p. 93.
Clinical Obs. on Malignant Dis. of Uterus (Barker), p. 1.
Cautery, Actual in, " " " " p. 23.

D.

Dalton, J C. Observations on Trichina Spiralis, p. 1.
Deformity of Pelvis, Management of, with induced Premature Labor (G. T Elliot), p. 24.
Dilators, Barnes', for inducing Premature Labor (Elliot),-p. 19 to 22.
Disease, Bright's, by Gouv. M. Smith, p. 1.
Disease, Malignant, of Uterus. Clinical obs. on (Barker), p. 1.
Douche, The, to induce Premature Labor (Elliot), p. 9.
Draper, J. C. Experiments on the Insensible Perspiration, p. 18.

E.

Elliot, G. T. On the Induction of Premature Labor, p. 1.
Edes, Rob't T. O'Reilly Prize Essay on the Phys. and Path. of the Ganglionic and Symp. Nervous Systems, p. 1 *et post.*
Etiology of Bright's Disease, with remarks on the Prophylaxis, by G. M. Smith, p. 1, 10.

INDEX.

F.

Female, On Sterility in the, by J. Kammerer, p. 1.
——————————— Treatment of, p. 7.
Fishbone, Removal of, by Laryngo-tracheotomy, after 14 years (G. Buck), p. 13.

G.

Ganglionic Nervous System., Phys. and Pathl. of (Prize Essay of R. T. Edes), p. 1.

I & J.

Injection of Chloride of Sodium into the Peritoneal Cavity in Septcaema after Ovariotomy (Peaslee), p. 91.
Insensible Perspiration, Experiments on, by J. C. Draper, p. 19.

K.

Kammerer. On the Pathological Conditions of Sterility in the Female, p. 1

L.

Labor, Premature, Induction of (G. T. Elliot), p. 1.
——————— Measures most Efficient in Producing Full Dilatability of the Cervix Uteri, p. 2 of Essay.
——————— The Douche in, p. 9.
——————— Barnes' Dilators in, p. 19, 22.
——————— With Deformity of Pelvis, p. 24.
Larynx, Removal of a Fish-bone from, after 14 years (Buck), p. 13.
Lidell, John A. Memoir on Osteo Myelitis, p. 1.

M.

Measurement of Deformed Pelvis, in connection with Induction of Premature Labor (Elliot), p. 24.
Malignant Diseases of Uterus, Clin. Obs. on some of the (F. Barker), p. 1.
——————————— Classification of, p. 7.
——————————— Symptoms of, p. 8.
——————————— Diagnosis of, p. 11.
——————————— Duration of, p. 15.
——————————— Treatment of, p. 15.
——————————— Abstract of Mortality of in City of New York (Appendix), p. 27.
Myelitis Osteo-, Memoir on, by John A. Lidell, p. 1
——————— Varieties of, p. 7.
——————— Path. Anatomy of, p. 15.
——————— Symptoms and Diagnosis of, p. 18.
——————— Terminations, Complications and Consequences of, p. 24.
——————— Treatment of, p. 26.
——————— " Operative of, p. 30.
——————— Exarticulation in, p. 31.

INDEX.

O.

Osteo-Myelitis, Memoir on, by John A. Lidell, p. 1.
Ovariotomy, Peaslee on, p. 1.
——————— Origin of, p. 36.
——————— Is it a legitimate operation in Surgery? p. 39.
——————— Statistics of, p. 46.
——————— To what class of cases adapted? p. 51.
——————— Causes of death in those who die from, p. 53.
——————— To be delayed until the general health is somewhat impaired, p. 57.
——————— Age, Temperament, Condition best adapted to, p. 62, 64.
——————— Conditions forbidding, p. 64.
——————— Operation of, how to be performed, p. 66.
——————— Preparatory arrangements for, p. 66.
——————— Precautions to be taken previous to, p. 66.
——————— Ether to be preferred in, p. 70.
——————— The operation of, itself, p. 70.
——————— Management of Pedicle in, p. 75.
——————— Dressing of Incision in, p. 84.
——————— Complications in the operation for, p. 86.
——————— Appropriate Treatment after, p. 88.
——————— Conclusions as to, p. 93.

P.

Passages, Air, Contributions to Surg. Therapeutics of (Buck), p. 1.
Perspiration, Insensible, Experiments on, by J. C. Draper, p. 19.
——————————— Effect of Ingestion of Food on, p. 31.
——————————— Conclusions as to, p. 34.
Peaslee, E. R. On Ovariotomy, p. 1.
Pedicle, The, in Ovariotomy. Tyler Smith's the best method of managing, p. 83
Pelvic Deformity, How to be measured (Elliot), p. 29.
——————— What indications for treatment? p. 33.
Premature Labor, Induction of, (Elliot) p. 1.
——————— with deformity of Pelvis, p. 24. (See Labor, Premature, Induction of, in Index.)
Prophylaxis of Bright's Disease, Remarks on (G. M. Smith), p. 1 *et post.*

R.

Relations of Sensible and Insensible Perspiration to each other (Draper), p. 24.

S.

Sanctorio, on the relations of Sensible and Insensible Losses of Perspiration to each other (Draper), p. 26.
Scirrhus of Uterus (Barker), p. 3.
Seguin's Experiments on Perspiration, p. 28.

INDEX.

Septicaemia, the most frequent cause of Death, after Ovariotomy (Peaslee), p. 90.
Smith, Gouv. M. Etiology and Prophylaxis of Bright's Dis., p. 1.
Sodium, Chloride of. Injection of into the peritoneal cavity in Septicaemia following Ovariotomy (Peaslee), p. 91.
Sterility in the Female, Pathol. conditions of (Kammerer), p. 1.
Surgical Therapeutics of Air Passages, Buck on, p. 1.

T.

Trichina Spiralis, Dalton on, p. 1.
Treatment appropriate after Ovariotomy (Peaslee), p. 88.
——— of Scirrhus Uteri (Barker), p. 15.
——— local, of " p. 22.
——— palliative, " p. 25.
Therapeutics, Surgical, of Air Passages, contribution to (Buck), p. 1.
Tube, Removal of a, from the Trachea (Buck), p. 1.

U.

Utèrus, Clinical Obs. on the Malignant Diseases of the (Barker), p. 1.
——— Scirrhus of the, " p. 2.

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Barry, Robert A.
Barstow, J. W., Phys. to Flushing Insane Asylum.
Bayles, George
Belden, E. B.
*Batchelder, J. P., April 7, 1868, aet. 82
Beadle, Edward L.
Beales, J. C.
*Beales, Gorham, January, 1848.
Beard, Geo. M.
*Beck, John B., April 9, 1851.
*Bedford, Gunning S., Sept: 5, 1870, aet. 64.
Bell, A. N., Physician to Brooklyn City Hospital.
Bell, C. M.
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Billington, C. E.
Blakeman, Wm. N.
*Bliss, James C., Aug. 12, 1855.
Bliven, J. P.
Blois, Samuel.
Blume, Samuel.
Blumenthal, Mark.
*Bolton, Jackson, Feb. 16, 1866, aet. 51
*Booraem, Augustus C., Dec. 16, 1871, aet. 46
*Boyd, Thomas, March 18, 1856.
Bozeman, Nathan.
*Brady, Patr. J., Oct. 24, 1856.
*Brooks, Geo. W.
Brown, D. T., Physician to Bloomingdale Insane Asylum.
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Bumstead, F. J., Ophthalmic Surgeon, N. Y. Eye and Ear Infirmary.
Burke, John.
Burrall, Fred. A.
Burtsell, Thomas E.
Byrne, John.
*Cameron, James, Dec. 12, 1851.
*Cammann, G. P., Feb. 14, 1863, aet. 59.
*Campbell, James, Mar. 12, 1853.
Caro, Salvatore.
*Carrington, Wm. A., July 17, 1866.

* Deceased.

*Carter, Galen, March 2, 1870, aet. 51
Chadsey, A. J.
Chalmers, Thos. C.
Chamberlain, W. M., Physician to Charity Hosp.
*Chapin, John R., June 23, 1852, aet. 41.
Chauveau, J. F.
*Cheesman, J. C., Oct. 11, 1862.
Cheesman, T. M.
*Childs, Timothy, Sept. 3, 1865.
Church, Allen S.
*Churchill, C. W., Oct. 27, 1859.
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*Conant, D. S., Oct. 8, 1865, aet. 40.
*Conolly, J. J., June 18, 1872, aet. 36.
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*Covel, John C., Nov. 4, 1860.
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Douglas, J. Hancock.
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*Drake, Benjamin, Jan. 11, 1871. aet. 65.
Draper, John C., Prof. Chem., Med. Dep. N. Y. University.
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Ellis, H. A.
Ellis, Sam. C.
Elsberg, Louis
Emmet, Thomas Addis, Surgeon, N. Y. State Women's Hosp.
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Farnham, H. P.
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Foster, Joel.
Foster, S. Conant.
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*Francis, John W., Feb. 8, 1861, aet. 72.
Francis, Samuel Ward.
Frankel, Edward.
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Frothingham, Wm.
Fuller, R. M.
Furman, Guido.
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*Green, David, Oct. 18, 1856.
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*Greene, Isaac, July 3, 1854.
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Griswold, Henry.
Guernsey, Peter B.
*Gunn, A. N., Dec. 21, 1871, aet. 62.
Hackley, Charles E.
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Hall, Samuel.
*Halsted, J., April 10, 1856.
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*Harsen, Jacob, Dec. 31, 1862, aet. 55.
Hart, Charles A.
*Hart, J., Aug. 9, 1867, aet. 57.
*Hazlett, John, March 4, 1829, aet. 85.
Hedges, David A.
Henry, M. H.
*Henschel, Chas., Sep. 18, 1872, aet. 64.
Herrick, Everett.
Herzog, Max, Physician to German Dispensary.
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Hogan, E. J.
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* Deceased. † Suspended.

Jacobi, A., Clinical Prof. Diseases of Children, Coll. Phys. and Surg.
Janes, Edward H., City Sanitary Inspector.
Janvrin, Joseph E.
Jenkins, J. Foster.
*Johnson, W. J., Sept. 22, 1860.
*Johnston, F. U., Jan. 7, 1858.
Jones, E. Lee.
Jones, Wm. W., Physician to St. Luke's Hosp.
Jones, A. S.
Kammerer, Joseph, Physician to German Dispensary
*Kearny, Ravaud, —— 1850.
Keene, Stephen S.
Kennedy, James.
Kerrigan, Joseph A., Physician to St. Vincent's Hospital.
Kilbourne, J. Sage.
*Kimbark, Everett H., Aug. 29, 1872, aet. 53.
*Kingsbury, G. H., May 4, 1852.
*Kissam, R. S., Nov. 28, 1861.
Krackowizer, E., Surgeon, Mount Sinai Hospital.
Lawrence, J. S.
Leale, Charles A.
Lee, Charles C., Surg. Charity Hospital.
Leaming, James R., Phys. St. Luke's Hospital.
*Leo Wolfe, George, March 14, 1855.
Leo Wolfe, Morris.
*Leveridge, Benj. C., April 16, 1862.
Lewis, William B.
Lidell, John A.
Linsly, Jared, Consulting Physician to Asylum for Lying-in Women.
Little, Jas. L., Surgeon to St. Luke's Hospital.
Livingston, W. C.
Loines, J. P., Physician to Eastern Dispensary.
Loomis, Alfred L., Prof. Theory and Practice, Med. Dep., N. Y. University.
Loring, Edward G., Jr.
Lusk, W. T., Prof. Obstet., Bellevue Hosp. Med. Coll.
*Lyon, Jas. L., Dec. 24, 1858.
MacGregor, J. R.
*Manley, Jas. R., Nov. 21, 1851.
Markoe, Thos. M., Surgeon, N. Y. Hospital, Prof. Surg. Coll. Phys. and Surgeons.
*Martin, Joseph, Apr. 26, 1864.
*Marvin, Dan. D., Oct. 21, 1852.
Maxwell, W. H.
*McAllister, G.
McClellan, Christopher R,
McClelland, John.
McCready, B. W.
*McDonald, Jas., May 5, 1849.
McDonnell, Patrick W.
McLeod, S. B. Wylie, Physician to Asylum for Lying-in Women.
McMillan, Chas., Physician to Orphans' Home, etc., Prot. Epis. Ch.
*McNeven, W. H., May 12, 1854,
McNulty, John.
*Meikleham, D. S. Nov. 20, 1849.
Metcalfe, John T., Consulting Physician to Bellevue Hospital
Miller, Francis.
*Miller, John, Jan. 13, 1863, aet. 57.
*Miller, W. Ellison, Jan 16, 1852.
*Miner, W. W., March 20, 1863.
*Miner, William, Nov. 16, 1859,
Mitchell, C. L.
Monell, J. A.
Monell, Joseph S.
*Moore, Sam. W., Aug. 26, 1854.

*Deceased.

*Moran, Thomas, —— 1853.
Morris, Moreau.
Morris, S. F.
Morton, Jeremiah C.
*Mott, Valentine, Apr. 26, 1865.
Neftel, W. B.
Nelson, James B.
*Neilson, John, June 19, 1857.
Nichols, Truman.
Nichols, Elias S.
Noeggerath, E., Physician to St. Franciscus Hospital.
Nott, J. C.
Noyes, H. D., Surgeon, N. Y. Eye and Ear Infirmary
*Ogden, Benjamin, June 18, 1867, aet. 70.
Ordronaux, John.
Osborne, Charles H.
Otis, F. N.
O'Meagher, Wm.
*O'Reilly, John, Dec. 6, 1868, aet. 56.
O'Sullivan, R. J., Physician to Eastern Dispensary.
Packard, C. W., Physician St. Luke's Hosp.
*Palmer, L. V.
Pardee, Charles I.
Parker, Willard, Prof. Surg., College of Phys. and Surg.
*Parkinson, W. B., May 11, 1856.
*Paul, James C., May 5, 1852.
Paine, Martyn, Prof. Mat. Med., etc., Med. Dep. N. Y. University.
Peaslee, Edmund R., Surg. to N. Y. State Women's Hosp.
*Pennell, Rich., Apr. 11, 1861.
Peters, J. C.
Peters, Geo. A., Surgeon to N. Y. Hospital.
*Phillips, Samuel B., Mar. 3, 1857.
Pinkney, Howard.
*Piatt, Wm. F., May 6, 1848.
Pomeroy, O. D. Physician to Northern Dispensary.
Pond, Jas. O.
Pooley, James H.
Pooley, Thomas R.
*Porter, Mort. G., Nov. 24, 1863, aet. 37.
Post, A. C., Prof. Surgery, Med. Dep. N. Y. University.
Post, W. H. B.
*Power, William, Sept. 14, 1858
*Pratt, Peter, —— 1860.
Prince, C.
Purdy, A. E. M.
Purdy, Alfred S.
Purdy, James W.
Purdy, S. A.
Purple, S. S.
Putnam, F. A.
Ranney, H. D.
Ranney, J. W.
Ranney, L.
*Ranney, M. H., Dec. 7, 1864.
Randolph, Israel.
Raphael, B. I., Surgeon, Mount Sinai Hospital.
*Ray, Robert, Jr., July 3, 1860, aet. 27.
*Reese, David M., May 13, 1861
Reynolds, James B., Phys. to Nursery and Childs' Hospital.
Richards, J. W.
Roberts, Wm. C.
*Robeson, A. B., Mar. 22, 1853.
Robie, J. W.
Rockwell, A. D.
Rodenstein, Louis A., Physician to N. Y. Institution for Deaf and Dumb.
*Rodgers, J. K., Nov. 9, 1851.
*Rogers, J. S., Mar. 29, 1851.
Rogers, Stephen, Surg. to Demilt Dispensary.
Roof, Stephen W.
Roosa, D. B. St. John, Aural Surgeon, N. Y. Eye and Ear Infirmary.

* Deceased.

Rosenberg, Emil, Physician to German Dispensary.
Ross, James, Physician to N. Dispensary.
Russell, C. P., Register of Vital Statistics.
Sabine, Gustavus A., Consulting Physician to N. Y. State Woman's Hospital.
Satterlee, R. S., Brig.-General, U.S.A.
Sayre, L. A., Prof. Surg., Bellevue Hosp. Med. Coll.
*Schilling, E., April 25, 1872, aet. 63.
Schirmer, Wm.
*Schmidt, John W., Jr.
Sell, E. H. M.
Sewall, John G.
*Shanks, John, August 10, 1870, aet. 70
*Sheppard, J. W.
*Sherwood, Burritt, Aug. 10, 1854.
Sims, J. Marion, Surg. to N. Y. State Women's Hospital.
Sloan, W. J., Surgeon, U.S.A.
*Smith, David, Jan. 17, 1867, aet. 57.
*Smith, D. A., April 5, 1872, aet. 29.
*Smith, Gilbert, July 16, 1851.
Smith, G. M., Physician to N. Y. Hospital.
Smith, Hanbury.
Smith, James O.
Smith, Jerome C.
*Smith, Joseph M., Prof. Mat. Med., etc., Coll. Phys. and Surg., April 22, 1866, aet. 77.
Smith, J. Lewis, Physician to Charity Hospital.
Smith, Oscar G.
Smith, Stephen.
Snelling, F. G.
*Snowdon, John, Jan. 1848.
Speir, S. F.
Spreng, Justus J.
*Spring, Edward, Feb. 13, 1850
Squibb, E. R.
*Stearns, John, March 18, 1858.
*Stephenson, Mark, August 28, 1865, aet. 62.
Stephenson, Marcus P.
*Stevens, Alex. H., Prof. Emeritus Clin. Surg., Coll. Phys. and Surg., March 30, 1869, aet. 80.
*Stewart, James, Sept. 12, 1864.
*Stickney, J. Dwight, Sept. 30, 1849.
Stiles, R. Cresson, Consulting Physician, Kings Co. Hospital
*Stimpson, E. B., May 15, 1858.
Stirling, Thomas B.
Stone, John O.
Storer, Ebenezer.
*Sweeny, Hugh, Sept. 15, 1857.
*Sweeny, James, —— 1872.
*Swett, John A., Sept. 18, 1854.
Swift, Foster.
*Swift, H. S.
*Taft, Marcus L., Feb. 8, 1850, aet. 29.
Tauszky, Rudolf.
Taylor, Charles F.
Taylor, Isaac E., Pres. and Prof. Obst., etc., Bellevue Hosp. Med. Coll.
Teats, Sylvester.
Teller, Seligman, Physician to Mount Sinai Hospital.
Tellkampf, Theodore A.
*Thayer, Henry W., —— 1857.
Thebaud, J. S.
Thoms, W. F.
Thomas, T. Gaillard, Prof. Obst., Coll. Physicians and Surgeons.
Thomson, W. H., Physician to Charity Hospital.
Thompson, B. S.
Thompson, George.
Townsend, John F.
*Townsend, Peter S., Mar. 26, 1849.

* Deceased.

Tucker, C. P.
*Tucker, Geo. H., Jan. 25, 1863, aet. 35.
*Tuttle, John T., Jan. 27, 1870, aet. 68.
*Uhl, David, Sept. 17, 1858.
Underhill, Alfred, Physician to Orphans' Home, etc., Prot. Epis. Ch.
*Vaché, Alexander F., June 9, 1857.
Van Arsdale, Henry.
*Van Arsdale, Peter.
Van Buren, Wm. H., Prof. Principles of Surgery, Bellevue Hosp. Med. Coll.
Van Buren, P.
*Van Buren, Thomas, —— 1848.
Vanderpoel, Edward.
Vandervoort, John L., Librarian, N. Y. Hospital.
Vanderveer, J. H.
Van Doren, M.D.
Van Kleek, John R.
Van Pelt, M.D.
Van Winkle, Ed. H.
Varick, Theodore R.
*Vedder, Joseph H., July 18, 1864.
Vermilye, Wm. E.
*Von Roth, Woldemar, —— 1857.
Walser, Theo.
Warner, E. B., Physician to N. Dispensary.
*Washington, J. H., Aug. 30, 1847.
*Watson, John, Jnne 3, 1863.
*Watts, Robert, Prof. Anat., Coll. Phys. and Surg., Sept. 8, 1867, aet. 55.
Weber, Leonard.
Weeks, Cyrus.
Weir, Robert F., Surgeon to St. Luke's Hospital.
Weisse, Faneuil D.
Wells, Ovid P.
*White, Ambrose L., Jan. 2, 1865, aet. 61.
White, Francis V.
White, Oliver.
*White, Samuel P., June 6, 1867, aet. 66.
White, William T., Surgeon to Demilt Dispensary.
Whitehead, Wm. R.
Wilkes, George, Consulting Surgeon, New York Eye Infirmary.
Williams, M. W.
*Winchell, M. E., May 1, 1864, aet. 35.
Winston, Gustavus S.
*Wood, Isaac, Consulting Physician to Bellevue Hospital, March 25, 1868, aet. 75.
Wood, James R., Prof. Surg., Bellevue Hosp. Med. Coll.
Wood, Stephen.
Woodhull, Henry W. B.
*Woodward, G. F.
Woolley, J. V. S.
Worster, Joseph.
Wright, Charles.

* Deceased.

NON-RESIDENT FELLOWS.

*Francis E. Berger, Paris.
*Charles A. Lee, Fishkill, N.Y., Feb. 14, 1872, aet. 71.
F. Campbell Stuart.
*H. Van Arsdale, Morristown, N. J.
J. King Merritt, Flushing, L. I.
R. S. Olmstead.
A. L. Sands.
F. U. Johnston, Cooperstown, N.Y.
J. W. Corson.
J. C. Hepburn.
Nelson L. North.
George F. Shrady, Barrytown, N.Y.

* Deceased.

OBSERVATIONS

ON

TRICHINA SPIRALIS.

BY JOHN C. DALTON, M.D.,

PROFESSOR OF PHYSIOLOGY AND MICROSCOPICAL ANATOMY IN THE COLLEGE OF PHYSICIANS AND SURGEONS OF NEW YORK.

Read before the New York Academy of Medicine, March 16, 1864.

DR. JOHN C. DALTON gave an account of some observations which he had lately made on Trichina Spiralis, and stated that his attention was more particularly directed to the study of the parasite, by the following report of a case of poisoning, as published in the *N. Y. Daily Times* of February 5, 1864:

"A case of much interest has just been concluded by Coroner NAUMANN. As we regard it of some public importance we give below the facts as they have come to us, together with the testimony adduced on the inquest, including evidence of the physicians, who have made a thorough examination of the subject. It appears that three weeks ago yesterday, an elderly lady, Mrs. ELIZABETH WERKMEISTER, residing at No. 45 Elizabeth street, purchased a ham at the store of a provision dealer on the corner of Grand and Mott streets. She took it home and the family all partook of it. Mr. FLAIG, the husband and father of the family, his wife, their little daughter MARY, aged two years, and an aunt of the latter, soon after eating, became quite ill. The next day, however, they ate some more of the ham. This second repast made them worse, and they began to attribute it to what they had eaten. The child lingered several days in great distress, and at length died, in spite of all the attending physicians could do for her. The father, mother, and aunt are still very ill, but hopes are entertained of their recovery. Mrs. WERKMEISTER, mentioned above, and who is the grandmother of the deceased, was not so much affected, she having only eaten a small piece to test it. Below we give the testimony of the grandmother and of the physicians who made the examination.

"*Elisabeth Werkmeister* being sworn, testified: Deceased was my grandchild; three weeks ago Thursday, I bought a ham, corner of Grand and Mott streets; I brought it home, and we all ate of it raw, and took bread and wine with it; on the next morning deceased's father and mother were

taken very ill; they complained of a great pain in the stomach, &c.; on that day they ate more of the ham, and since that time they have been constantly very ill; they thought the ham poisoned them, and I ate a piece of it to try it; the same symptoms showed themselves with me; afterwards the deceased was taken sick, and Dr. Valliere was sent for.

"*Dr. Valliere*, residing in Jackson street, testified: I am a physician; two weeks ago to-day I was called to see deceased; I found her in great pain, vomiting, &c.; these symptoms continued for four or five days; the deceased gradually grew weaker, and died day before yesterday; all the members of the family were similarly affected; the family consisted of the father, mother, grandmother, aunt, and deceased; they were all of the opinion that the sickness was caused by eating a ham, which they had purchased a short time before; on examining the ham with a microscope it was found to be diseased, and filled with a kind of insect known as '*trichina*.'

"*Joseph Schnetter*, of No. 99½ Amity street, testified that he was a physician; that he saw deceased in consultation with Dr. Valliere; deceased was suffering from symptoms described by the doctor; my opinion was that the disease was caused by the '*trichina spiralis*;' I took a piece from the same ham that deceased had eaten, and examined it under a microscope; I found that it contained an immense number of the animalcules; as far as the appearance of the ham with the naked eye went, it seemed to be healthy; nothing wrong could be detected with the naked eye, and no blame could be attached to the dealer for selling the article. I have seen several cases of disease from the same cause, and believe it to be more prevalent this season than others.

"*Dr. Wooster Beach* testified: I have made a post-mortem examination of the body of deceased, with Drs. Valliere, Schnetter, and Jacobi; the organs showed no marks of disease, except the brain and left lung, which exhibited signs of congestion; portions of various parts of the body were removed for microscopic examination; I agree with Dr. Schnetter in his opinion as to the cause of the death of deceased.

"At the conclusion of the testimony, the jury rendered the following verdict: 'We find that the deceased, Mary Flaig, came to her death from accidental poisoning from eating ham containing *trichina spiralis*.'"

Dr. Dalton then proceeded:

The existence of Trichina spiralis has been known for about thirty years. For a long time, however, it was supposed to be peculiar to the human subject, and all that was definitely known, with regard to its locality, was that it was confined to the voluntary muscles. It has often been found in the muscles of dissecting-room subjects, and I have had in my possession for the last ten years a fine specimen of the rectus femoris muscle, from a dissecting-room subject, which is filled with these parasites. In this and similar specimens the parasites are inclosed in cysts, which are very easily recognised by their appearance, and are even visible to the naked eye, notwithstanding they are only $\frac{1}{50}$ of an inch long, and $\frac{1}{100}$ of an inch in thickness; this, however, is owing to the fact that the cysts, in these speci-

mens, are filled with a calcareous deposit, which gives them a dense opaque, whitish appearance.

The cysts are somewhat shuttle-shaped, swollen in the middle and tapering at the extremities. The worm, which has a very characteristic contour, being coiled spirally upon itself, is found in the central part of the cyst; while the deposit of calcareous salts occupies the extremities.

In all instances, until recently, in which these worms had been found, notwithstanding that all the voluntary muscles, even to the muscles of the larynx and those of the middle ear, were infested by them, it was never known that they produced any injurious effects. Indeed, in some instances, persons, in whom this state of things existed, were positively known to have been in the enjoyment of good health before death. The trichina was supposed to be a harmless parasite.

I must confess that this was the extent of my own knowledge in reference to them, when I saw the account, which I have just read, of a family being poisoned by them. By the kindness of Dr. Voss I came in possession of a part of the ham which was the cause of the poisoning in this case, and on examining it by the microscope I found, as Dr. Schnetter had stated, that it was filled with trichinæ. But these parasites, in their microscopic appearances, were somewhat different from those with which I was already familiar in the human subject.

The first thing which struck me was their quantity. In a piece of muscle, $\frac{1}{12}$ inch square and $\frac{1}{25}$ of an inch thick, where they were in average abundance, I counted twelve trichinæ. This would give, in round numbers, over 85,000 to the cubic inch. In this ham the trichinæ were inclosed in cysts, as in other cases, but these cysts differed from those found in the human subject in not being the seat of calcareous deposits. Hence they were nearly as transparent as the muscular fibres themselves, and for this reason, as well as for being somewhat smaller, they were not visible to the naked eye. The cysts in the ham were $\frac{1}{25}$ of an inch long, and $\frac{1}{125}$ of an inch in thickness. They were also peculiar in having long caudate processes, or prolongations, running out from their two extremities. In the human subject the cysts terminate by rounded extremities, which are perfectly distinct, but in the ham these extremi-

ties taper off into long processes, which are of such length that I have never been able to find their ends. In tearing

Fig. 1.

Trichina spiralis, in Cysts, from Muscular Tissue of Ham.

the muscular fibres apart for microscopic preparation, these filamentous prolongations are torn off at various distances from the cyst, and this is not to be wondered at when we remember how closely they are interwoven with the muscular tissue (Fig. 1). It requires considerable care to extricate a cyst even with one of these prolongations undetached. Such a one is represented in this drawing (Fig. 2).

The cyst is spindle-shaped as in the human subject, with the worm coiled up in its central part, but merges at once from each extremity into its prolongation. The structure of the

Fig. 2.

Trichina-Cyst, from Ham.

cyst is peculiar. It consists of a hollow sac; but the cavity of the sac does not communicate with the interior of the prolongations. The walls of the sac are tolerably thick, but transparent and colorless; and at each end of the cyst its cavity terminates by a rounded cul-de-sac. In its interior there is a deposit of granular matter, which makes the outline of the cavity tolerably distinct, while at the same time it somewhat obscures the outlines of the worm itself. Sometimes by pressure the sac may be ruptured and the worm expelled, and then it is easy to see that it has a distinct cavity, which was previously occupied by the worm imbedded in a soft granular material (Fig. 3). The nature of the prolongations of the cyst was a matter of great interest to me, and the first specimen,

[Fig. 3.

Trichina-Cyst, from Ham, broken open and empty.

which I had the opportunity of examining from the ham, showed several facts not described by previous observers. In the first place, after some care, it was found that these prolongations are invariably present at both extremities of the cyst. Some writers occasionally speak of the cysts as sometimes having one prolongation and sometimes two, but in this specimen the latter was always the case. Secondly, the prolongation is *tubular in character*. A granular matter fills its cavity in the same way as that of the cyst, but in the tubular prolongation it is less dense and more transparent. This granular matter appears in most instances gradually to disappear as we approach the cyst, but in favorable cases it may be seen to

come up nearly to the cavity of the cyst, and is in reality simply separated from it by a perfectly defined wall or partition, of the same thickness and appearance as the wall of the cyst itself. (Figs. 3 and 5.) In some instances I have seen even a second cavity, beyond the end of the cyst, shut off also from the tube by a second transverse partition. I have also noticed, in several instances, that by pressure upon the unruptured cyst the transverse partition which separates the two cavities may be broken down, and the granular contents of the cyst forced out into the cavity of the tubular prolongation. Then, with regard to the character of the cyst wall and the wall of the tube, they appear to be without definite structure; for if a portion of the lacerated cyst wall be examined with a high power, it presents only a very finely striated appearance, similar to that seen in fibrinous exudations, but does not show any indication of a definitely fibrous texture.

If the trichina be expelled from the cyst by pressure, and examined by itself, it is seen to be a round worm, $\frac{1}{12}$ of an inch long, and $\frac{1}{1215}$ of an inch in thickness. In the cyst it lies coiled up in three turns, so that the tail is opposite to, and very near the head; but after extricating it from the cyst, I have often been able to unwind it sufficiently for purposes of measurement and examination. It has an anterior extremity which is rather pointed and narrow, and a posterior extremity which is comparatively thick and blunt. It has a very thin and transparent, but strong and elastic integument, which is covered with exceedingly minute annular grooves, which give a finely serrated look to its free edges. It has an alimentary canal, which commences by a mouth at the anterior extremity, followed by a narrow œsophagus, which soon dilates into a large intestine, occupying nearly the whole thickness of the body of the animal. About two-thirds of the distance from the mouth to the anus, this intestine very suddenly diminishes in size, then enlarges again into a kind of pouch, and then diminishes again to a narrow tube, which continues, following the curve of the body, until it reaches the posterior extremity, where it communicates with the anus. In the posterior third of the worm, besides this smaller part of the intestine, there is another body, with regard to the nature of which there has existed some

FIG. 4.

TRICHINA SPIRALIS, FROM HAM, MAGNIFIED 210 TIMES.

doubt. It is a wide tube, having no apparent opening in either direction, which occupies most of the thickness of this part of the worm, and is very distinct in consequence of being filled with a large number of highly refractive, oleaginous-like globules. This body is supposed to be the germ or origin of the generative apparatus; but if so, it is entirely undeveloped and imperfect, as there are no signs of any distinct sexual organs. Indeed, one of the most interesting circumstances in regard to this parasite has always been the fact that it is sexless, and its existence, therefore, difficult to account for. These, then, are the most important points connected with the anatomy of the parasite. There are various other accounts given by different observers in regard to its structure, but I have only

referred to those particulars of which I have satisfied myself by direct observation.

A very difficult point, and one which I found it impossible to settle from an examination of the first specimens that came under my notice, is that regarding the exact *location* of these worms. What is the nature of this tubular cyst in which they are contained? Usually one worm alone occupies the cavity of a cyst, but occasionally there are two together (Figure 5).

Fig. 5.

Trichina-Cyst, with Two Worms, from Ham.

One specimen from the ham showed a very curious pair of cysts, running continuously into each other, by means of their prolongations (Figure 6). It was very evident from the examination of these and similar specimens, that the worms were con-

Fig. 6.

Trichina Cysts, connected, from Ham.

tained, either in the cavities of bloodvessels very much altered from their normal condition, or in the interior of the muscular fibres themselves. So long as these parasites were only found in the human subject, it was supposed that their cysts were either egg-membranes belonging to the worm, or products of exudation thrown out from the muscular tissue. But the tubular prolongations which I have described are evidently nothing which belongs to the worm itself, and must therefore be derived from the tissues in which they are found.

The cyst, with its prolongations, must therefore be one of three things. Either, 1st, an inflammatory exudation thrown out immediately around the worm and along the track through which he has come; or 2d, a muscular fibre with its sarcolem-

ma thickened and its contractile matter degenerated and atrophied; or 3d, a bloodvessel thickened and distended at the situation of the worm, and collapsed and partly solidified above and below. The first of these suppositions, however, cannot be the true one, since I have found the prolongations always at both ends of the cyst, and indefinite in extent; and though we might very readily imagine an exudation thrown out behind the worm, along the course which he has followed in order to arrive at his present situation, this does not explain why there should be a similar one, of the same extent, in *front* of him. The cyst must therefore be either a muscular fibre or a bloodvessel. Both Leuckart and Virchow assert it to be a muscular fibre, in the interior of which the worm establishes himself, after arriving in the muscular tissue. The appearances which I have seen in later observations lead me to believe that it is a bloodvessel, by which the worm has been brought to his present situation, and in which he is finally fixed at a particular spot.

The pathological relations of the worm are of still greater interest. Dr. Schnetter, of Amity street, who is very familiar with this subject, informs me that he has had a previous case, of his own, of undoubted poisoning by trichina spiralis. The case was that of two servant girls in the upper part of the city, who were taken sick about the 20th of January, shortly after having eaten a meal of pork steaks, which were very much underdone. Dr. Schnetter was so much convinced of the truth of his suspicions, that he made inquiry as to the condition of the rest of the family, and found that they had not partaken of the meat, and that they consequently had not suffered any inconvenience. One of the servant girls, shortly after having eaten the pork, went to live in another family, and on making inquiry, Dr. S. found that she too had been taken sick and suffered from the same symptoms as the first.

It seems that this trouble, from eating pork infested with trichina spiralis, which I think is somewhat new in this country, has been for the last four years tolerably well known in Germany, and is generally recognised there, as a disease to be looked for and diagnosticated.

Dr. Krackowitzer has kindly loaned me a recently published pamphlet by Virchow, in which he gives a full account of the

history of this disease since the year 1860, a short abstract of which was published in the AMERICAN MEDICAL TIMES for February 20th. It seems that not only is trichina found in ham and in all kinds of pork, but that its bad effect upon the human subject, when taken in the food, is fully recognised. In Germany the trichina disease has been noticed in quite a number of cases, and there have been no less than five distinct epidemics of it. The first case happened in Dresden, in 1860. A family partook of recently killed pork, and all the members, together with a servant girl, were taken sick with the peculiar symptoms belonging to this affection. The servant girl died. At the autopsy her muscles were examined and found to be full of the trichina. Virchow repeated the same examination, with the same result. He administered a part of these muscles to a rabbit as food, and the animal, who ate freely, died at the end of a month. The muscles of this animal were then given to a second rabbit, who died four or five weeks afterward; and the muscles of the second rabbit were then given to a third, which also died at the end of a month. In all these cases the muscles of the rabbits were found full of the trichina spiralis; thus showing beyond a doubt that the fatal effects were due to the trichina, and that the parasite could be propagated from one individual to another by being administered in the food.

One of the most remarkable of the German epidemics was that which happened in Hettstadt, in which nearly 150 people were affected, and over twenty died.

About the same time that my attention was directed to the existence of trichina in ham, Dr. Voss was kind enough to invite me to see a case of recent trichina disease on board one of the Bremen ships in our harbor. It seems that on this ship the captain, first mate, second mate, and cook were taken sick simultaneously, on the 5th of February. Dr. Betzendorf was called on the 8th, and, from the symptoms which were present, suspected the existence of trichina disease. These symptoms were abdominal pain and diarrhœa soon after eating of the suspected food, followed within a day or two by œdema of the face. At the time Dr. Betzendorf was called, the patients were suffering from anorexia, and some fever. Dr. Voss was then sent for in consultation. He was not able to trace the

illness to any particular article of food, unless it were some pork which had been eaten a short time before the patients were taken sick. In order to satisfy himself fully, however, as to the nature of the disease, Dr. Voss cut down upon the deltoid muscle of the second mate, and removed a portion of its tissue for examination. It proved to be filled with trichina spiralis. Dr. Voss was kind enough to give me a portion of the same muscle, and I also found in it an abundance of trichinæ, similar to those of the ham. They were not, however, quite so abundant, numbering, in the human muscle, only a little over 7000 to the cubic inch.

Another point, however, of great interest was elucidated by the examination of these specimens from the human muscle. It was very evident that although the trichinæ in the human muscle were the same animals with those in the ham, their cysts presented a very different appearance (Fig. 7). Instead of being contained in closed fusiform sacs, with narrow prolonga-

Fig. 7.

Trichina, in tubular Cyst, from recent Human Muscle, taken the 13th day of illness.

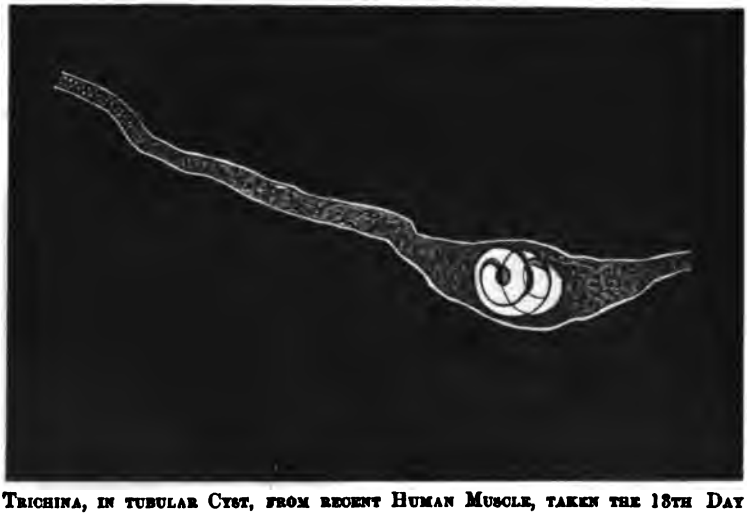
tions, the worms were here evidently in the cavity of continuous tubes, which were only slightly dilated at the point where the

parasite was lodged. There were no transverse partitions, shutting up the worm in a cavity of its own, but by proper manipulation and gentle pressure, the worm could be readily forced into the neighboring undilated portion of the tube, and back again into its wider part, as in the specimen shown in Fig. 7.

Usually, when undisturbed, the worm lay fully coiled up, as in the other specimens. The wall of this tubular cyst is much thinner than that in the specimens from the ham.

Other appearances in the wall and contents of this tubular canal lead one to the belief that it is nothing else than a dilated and altered bloodvessel (Fig. 8).

Fig. 8.



Trichina, in tubular Cyst, from recent Human Muscle, taken the 13th Day of illness.

Its cavity is filled with a granular material, which is much less opaque than that contained in the older trichina-cysts of the ham, and which appears to be the coagulum remaining in an obstructed vessel. There are collected at various points in this granular matter little bodies which can hardly be mistaken, and which are evidently more or less altered blood-globules. The wall of the tube also shows upon its internal surface thin flat nuclei, similar to those of the smaller bloodvessels.

From these observations, accordingly, it would appear that the

worm is actually contained within a bloodvessel, and that it is conveyed throughout the system and reaches the muscular tissue by the circulating current, and that it then becomes arrested, and finally stops the circulation in the vessel which contains it.

There is also another fact which corroborates this view of the case, and which is important as being one of the earliest symptoms in trichina disease. I refer to the œdema. This appears usually very early in the face, and soon after becomes general. It was well marked all over the body in the case from which Dr. Voss obtained the specimens which I have described. This œdema, which would be very difficult to explain on any other supposition, is easily understood by an arrest of the circulation taking place simultaneously in so many capillary bloodvessels, as must necessarily be obstructed, when 7,000 trichinæ are contained in the space of a cubic inch. These parasites must produce a retardation in the flow of blood, which is the true cause of the effusion of serum in the surrounding tissues. At the time Dr. Voss obtained the specimen mentioned above, the disease was in its second stage, the patient complained of muscular pains, and the anasarca was general.

It appears, therefore, from all these sources of information that the trichina is very common as a parasite in pork; and this circumstance, connected with the fact that it is not only communicable to the human subject, but that it gives rise to a very troublesome and fatal affection, is a matter of great importance for the general public as well as for the profession. It seems that the history of the ingestion and propagation of these parasites is as follows: The patient takes, for food, ham or pork, which is not thoroughly cooked. It is important to notice that *smoking* alone does not kill the trichinæ, even when it is very thoroughly done. Dr. Schnetter informs me that the ham which produced the sickness in the Elizabeth street family, was an American ham, thoroughly smoked, and not to be distinguished by anything in its appearance from any other ordinary ham. The prevalence of this disease, as well as that of the tapeworm, especially among the Germans, is undoubtedly owing to their habit of eating hams, sausages, and sometimes fresh pork in a raw state. At all events, it is thorough cooking

alone which will afford protection against poisoning by either of the parasites infesting this kind of food.

So long as the worms occupy the muscular tissue they are perfectly quiescent, and, as I have said, sexless. But as soon as they are taken into the stomach and the muscular fibres digested, the worms, according to the observations of Leuckart and Virchow, pass into the intestine, where they immediately begin to grow, attain rapidly three or four times their former size, acquire fully developed generative organs, and, in the course of a week to ten days, the female contains an abundance of living young. These young, as soon as discharged into the intestine, begin to work their way through the mucous membrane, penetrate the bloodvessels, are distributed all over the body—become arrested in the capillary bloodvessels of the voluntary muscles, obstruct these vessels, grow to a certain size, and there remain stationary. While penetrating in such large numbers through the intestinal mucous membrane, they produce abdominal pain and diarrhœa, which are the first symptoms of the attack. As soon as they arrive and become fixed in the muscular tissue, they cause universal muscular pains, often very severe, anasarca, anorexia, and fever of a typhoid character. In the fatal cases, death generally takes place at the end of four or five weeks from the commencement of the attack, and is due to the intensity of the febrile symptoms. The child in Elizabeth street, however, who was seen by Dr. Schnetter, died exhausted by the diarrhœa at the end of two weeks. If the patient survives the first few weeks of the attack, the muscular tissue accommodates itself to the presence of the inactive parasite, the obstructed vessel shrinks and becomes obliterated, while others in the neighborhood undoubtedly become dilated and fully restore the circulation. Fibrinous partitions are thrown across the obstructed vessel in the immediate neighborhood of the parasite, inclosing him in a shut sac, while the rest of the vessel becomes gradually converted into the narrow prolongations which I have described. After some years these prolongations even disappear altogether, and the worm is then inclosed in a simple fusiform cyst, with rounded extremities. Calcareous matter is in the meantime slowly deposited, particularly in the ends of the cyst, so that it becomes hard, white,

and opaque. This is the condition of those specimens usually found in dissecting-room subjects. Exactly how long they may remain in this state is not definitely known, but from a very curious observation made by Langenbeck in 1863, and mentioned in the article already adverted to in the *Amer. Med. Times* of February 20, it appears certain that they may remain perfectly recognisable for at least eighteen years in the human muscular tissue without producing any inconvenience, beyond the first few weeks after their ingestion.

The mystery heretofore attending the appearance of trichina spiralis is therefore partially cleared up. The propagation of these worms is analogous, to a great extent, with that of the cysticercus and tape-worm. The cysticercus occupies, in great numbers and in a quiescent and immature condition, the cellular tissue of the pig. The pig's flesh is eaten by a man, and in the human intestine one of the cysticerci becomes developed into a tape-worm, with fully formed sexual organs, and capable of producing living embryos. The fruitful and mature tape-worm-articulation, full of young, is thrown off and discharged from the intestine, and afterward devoured by a pig. In the pig's intestine the young embryos are set free, perforate the mucous membrane, enter the bloodvessels, are distributed over the body, and become fixed and encysted in the cellular tissue as cysticerci. There is a regular and necessary alternation between the immature cysticercus in the flesh of the pig, and the fruitful tapeworm in the human intestine.

In the same way the trichina remains immature while in the muscular flesh, and becomes fruitful only when introduced into the human intestine.

But a great difficulty has been still thought to exist with regard to the continued propagation of this worm. If the immature trichinæ of the infested pork become fruitful when eaten, and produce a new brood, and if this new brood immediately penetrate the mucous membrane and are distributed over the muscular system of the patient, there to become encysted and quiescent, how is a third brood ever to be produced? How is the parasite ever to get back again to the muscular flesh of the pig, there to continue its propagation on being eaten by a man?

I believe this difficulty, however, to be only an apparent one. We must not suppose because the young brood of some of the mature trichinæ are discharged in the human intestine, to penetrate through the mucous membrane to the muscles, that *all* of them follow this course. It seems more probable that a portion, if not most, of the fruitful trichinæ pass out of the intestine, like the tapeworm-articulation, with the fæces, and only discharge their living brood when subsequently eaten by the pig. If this be so, the infection of the human muscular system by trichinæ should be regarded as a kind of accident owing to the rapid development and discharge of some of the young broods before the adult worms have left the intestine. A similar occurrence happens sometimes with regard to the tapeworm. For though generally the mature articulations containing the young are discharged entire from the intestine, so that we only find cysticercus in the flesh of the pig and only tœnia in the human intestine, *occasionally* one, two, or even more cysticerci are found in the human subject, where they have been seen in the muscular substance of the heart, in the cellular tissue, in the eye, and in the brain.

In the case of trichina, the tendency of a portion of the new brood to find their way into the human muscles is much greater than in that of the tœnia, and constitutes the most annoying and dangerous circumstance connected with the disease. But so far as the physiological history of the parasite itself is concerned, this circumstance is altogether secondary. The history of the propagation of trichina spiralis, like that of tœnia and cysticercus, is that of a regular and natural alternation between the immature encysted sexless parasite in the pig's flesh, and the fully developed fruitful worm, reproducing its species in the human intestine.

EXPERIMENTS ON INSENSIBLE PERSPIRATION.

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Read before the New York Academy of Medicine, May 4, 1864.

The object of this paper is to demonstrate by means of experiments with a suitable balance—

First.—That the products arising from the destruction of muscular tissue during exercise are not excreted as urea by the urinary apparatus, but pass out of the body in the form of insensible perspiration through the skin and lungs.

Second.—That a definite period of time (usually about one hour) must elapse, after the muscles of the body have been actively employed, before the rate of insensible loss falls again to the normal standard.

Third.—That about one-half of the total amount of food and liquid consumed, passes out of the system as insensible perspiration.

Fourth.—To examine the variations in the amount of insensible loss from day to day during a period of many weeks, and to show the effect of ingestion of food.

During two years' service as physician in the New York and Bellevue Hospitals, I was frequently impressed with the importance of the skin as one of the great sewers of the body, and I determined that, when opportunity offered, its duties and functions should be made the subject of study and experiment.

To carry out this intention, in the fall and winter of 1862 I constructed a balance by which the weight of the body of an adult could from time to time be accurately determined and the

variations duly noted. Considering the size of the instrument, it was very sensitive, for when loaded with 200 pounds, the addition of one grain to the contents of either pan was instantly shown by a corresponding movement of the index. Indeed, the first difficulty with which I had to contend was the delicacy of the balance, for when it was adjusted to produce the result given above, it was impossible to use it with satisfaction; for the movement of the index was so slow, and the diminution in the weight of the body due to the insensible perspiration so rapid, that it was out of my power to arrive at a correct result. This was, however, remedied by reducing the sensibility in order to adapt the instrument to the work for which it was intended.

As regards the construction of the balance, it is sufficient to state that the beam was about six feet in length and two feet six inches wide at the middle, tapering gradually to the ends. It was supported at its centre on a frame-work, so as to be six feet above the floor. To one of the extremities of the beam a pan or platform was attached, on which the weights were placed, and to the other a sort of chair in which the subject of the experiment could sit in a perfectly natural, easy position as long as was necessary.

The beam and supporting frame-work were constructed of pine wood two inches thick, that they might be as light and strong as possible. The knife-edges at the extremities of the beam, on which the pan and chair were supported, and the axis or fulcrum at the centre, were made of the best hard-tempered steel, accurately ground to give in section an equilateral triangle measuring one inch on each side. The centre axis or edge was six inches long; those at the extremities five inches. The centre axis rested on a perfectly flat plate of hard, polished steel, while the pans were connected with the edges at the ends of the beam by means of the usual stirrup-shaped suspension pieces. In order to avoid any accident from slipping, a groove on a radius of one-quarter of an inch was cut in the foot-piece of the stirrup, and carefully ground to fit the knife-edge on which it was to be supported.

The weights employed were French, but if it is desired, the results can be readily converted into our Troy standard—1000 grammes being equal to 2·679 lbs. Troy.

Complications arising from variations in dress were avoided, by wearing in all the experiments the same clothes, the weight of which was known, and of which I shall in future speak as the weighing-dress. Errors arising from variations in the time of passing the sensible evacuations were also carefully avoided.

The method employed in weighing, was to place on the platform a counterpoise heavier than the person to be weighed. Equilibrium was then obtained by taking small weights into the hands while sitting in the opposite pan or chair of the balance; adding this to the weight of the dress employed, and subtracting the sum from that of the counterpoise, the weight of the body at the time of the experiment was attained. Any necessity for the presence of an assistant to watch the movement of the index was obviated by attaching to the frame of the balance a small silver reflector in which the index scale could be seen by the person sitting in the chair.

A suitable balance being thus provided, I proceeded to the investigation of various problems connected with the weight of the body; the first that is presented is the consideration of the

Diurnal and Nocturnal Loss by Insensible Perspiration.

In order to determine the relation existing between the diurnal and nocturnal losses by insensible perspiration (under which title I have included all the insensible evacuations of the body, not only those by the skin, but also those from the lungs), a series of experiments was made, which extended throughout the month of January, 1863. The average of the results obtained is shown in the following table:

	Insensible loss per minute.	Temp.	Dew Point.
Day rest,	·79 gramme.	55°	46°
Night sleep,	·47 "	50°	42°

In the above the nocturnal loss is that which takes place during quiet, placid sleep, and the diurnal that which occurs when the body is kept in as perfect a state of rest as is consistent with comfort, most of the time being spent either in writing in the sitting or reading in the recumbent posture.

To the observant, intelligent physician, who has for many years endeavored to control the condition of his patients by seeking to give them sleep as well as mere muscular rest, the

above results give the explanation of the wonderful effect which sleep has, not only in restoring the tone of the nervous system, but also in arresting the great loss of weight and emaciation which attends so many diseases during the restless, sleepless period, but which ceases almost the moment that a calm night, in which sound sleep has been obtained, is granted to the sufferer.

For during such a night the insensible loss which is continually taking place is only about one-half of that which occurs during simple muscular rest without sleep. A sound sleep usually marks the commencement of the stage of convalescence in any disease.

Having obtained in these experiments the average loss per minute, by insensible perspiration during the day, while in a state of rest, we shall regard it as the normal diurnal standard of loss, and proceed to examine the variations which muscular action produces.

To determine this, a number of trials were made, which demonstrate that in health increased activity in these organs is the chief cause of increase in the rate of loss by insensible perspiration; and that in moving the body of an adult weighing 65,000 grammes one mile, 44 grammes in addition to the usual amount are lost by insensible perspiration.

The data from which the above conclusions were deduced, were obtained by performing a series of experiments in which exercise, varying in duration and intensity, was undergone; and determining the increase in the rate of insensible loss, it was found to be forty-four thousandths of a gramme for every thousandth of a mile per minute of motion, as is illustrated by the following examples:

For 10-thousandths	mile	per	minute	motion,	1·16	gramme	per	minute	of	insensible loss.
" 19	"	"	"	"	1·67	"	"	"	"	"
" 22	"	"	"	"	1·88	"	"	"	"	"
" 41	"	"	"	"	2·40	"	"	"	"	"

These examples not only bear out the statement regarding the increase in loss which follows active muscular exertion, but they also show how great an influence muscular action has in

promoting the function of insensible perspiration; a movement of forty-one-thousandths of a mile per minute, which is equivalent to three miles an hour, causing the rate of loss to rise from 80 to 240. Could we have indicated to us more clearly the true channel through which the products of waste and decay in the interior of the economy during violent muscular action are thrown off?

In all our works on physiology it is the received doctrine that the urea of the urine represents the result of the disintegration of the muscular or chief nitrogenized tissue of the body. It follows, of course, that the proportion of this ingredient of the urine should increase greatly whenever the amount of muscular action is increased.

In my inaugural thesis for the degree of M.D., published in 1856, the experimental examination of this question was fully entered on, and by the use of new and more reliable methods for the determination of urea, it was demonstrated that exercise does not influence the proportion of this substance contained in the urine in the manner usually supposed, as will be seen in the following table:

For 24 hours.	Urine.	Solid residue.	Urea.	1000 solid residue contains of urea
Average loss	1106	55,584	27,213	490
Exercise	905	52,031	25,472	489

The diurnal amount of motion in the results given under the head of average loss, was about four miles a day; that under the title of exercise, was thirteen miles a day. If it is true that urea is the result of disintegration of muscular tissue, we are justified in expecting to find a very great increase in the amount of that substance when long-continued exercise is undergone; but, on the contrary, not only is the quantity of urea and the amount of urine and solid residue diminished, but the proportion of nitrogenized matter contained in the solid residue also undergoes diminution, thereby showing in the most conclusive manner that the products arising during the disintegration of muscular tissue do not find their way out of the system in the urinary secretion.

The experiments of Lehmann on the influence of variations in diet, and the consideration of the relations of the diurnal and nocturnal urine to each other, furnish additional support to

this opinion. Hammond also has shown that increased mental exertion is invariably accompanied by increased elimination of urea, demonstrating as he supposes the effect of the nervous system on the production of urea. But we may explain the increase by the fact that during mental exertion the body is kept in a comparative state of rest, in which condition I have shown that there is more urea excreted than during violent exercise.

Keeping in view the facts regarding the diminution of the nitrogenized ingredients in urine during violent muscular action, we have in part the explanation of the great increase in the loss by insensible perspiration under such circumstances. Nor is there anything surprising in the statement that the products arising during the disintegration of muscular tissue escape as insensible perspiration rather than by the kidneys, for we cannot imagine any channel through which it could take place more rapidly or in a more effective manner.

In support of the opinion that the effete substances arising during the destruction of muscular tissue escape by the skin or lungs rather than by the kidneys, let any one appeal to his own experience. We all have frequently, at various periods of our lives, undergone great muscular exertion, walked perhaps fifteen or twenty miles without resting for more than a few minutes, and passed but very little urine by the way, or even none at all; the journey ended, there was no great increase in the quantity of urine, and yet our reason tells us that there must have been a great destruction of muscle in order to move a weight of 120 to 200 pounds over a distance of many miles; and we are obliged to admit that there could not be a more suitable and perfect channel for the removal of the products of muscular waste than that of insensible perspiration, either by the skin or the lungs, nor a more favorable condition than the vaporous or gaseous state.

It may also be demanded how we can explain the great congestion of the skin which almost immediately supervenes on exercise, except by supposing that effete materials are being rapidly voided; and increased congestion is necessary in order to produce increased activity in any organ. The mere statement that the great flow of blood to the skin is only for the

purpose of reducing the temperature by evaporation, is not altogether satisfactory.

Organic tissues decaying or burning, oxidizing slowly or rapidly, disappear in the form of gases or vapors as carbonic acid, ammonia, and nitrogen. In animals we know that carbonic acid is given off, and the presence of ammonia, nitrogen, and other inorganic and organic compounds as constituents of the secretions both of the skin and lungs, has been demonstrated by Berzelius and other chemists. The organic compounds are especially to be noticed as causing the deathlike sickly odor which pervades the closed wards of a hospital, or which exists in the apparatus in which experiments on respiration have been conducted.

The consideration of these facts affords sufficient justification for advancing the opinion that the products of the disintegration of muscular tissue find their way out of the system, not through the urinary apparatus, but as insensible perspiration from the lungs and skin; and the great increase in the insensible losses caused by violent exercise is in great part due to the evacuation of the products of muscular waste through those channels.

While we are considering the evacuation of effete material in a form which is not appreciated by the senses, it becomes a matter of interest to determine whether the loss due to muscular action ceases as soon as the exercise ceases, or is continued for some time afterwards. In order to answer this inquiry, I made a series of experiments in the months of February and July, 1863. The rate of loss before exercise was first determined. I then walked on different occasions distances varying from one to five miles, and by weighing as soon as I returned, obtained the loss during the exercise. I then weighed at intervals of twenty minutes, so as to find at what time the rate of loss became the same as before the exercise was undertaken.

The results obtained showed that the rate of loss is increased for some time after exercise, and it is necessary that about an hour should elapse before the standard is regained; as illustrations, a couple of experiments are given. The average rate of loss per minute in the state of rest was ·79 grammes.

Rate of loss per minute after violent exercise.

No. I. First (20 minutes), 1·25. Second (20 minutes), ·91. Third (20 minutes), ·76 grammes.

No. II. First (20 minutes), 1·65. Second (20 minutes,) 1·00. Third (20 minutes), ·90 grammes.

Both of these experiments show a steady decrease in the rate of loss after the exercise ceased. In the first, at the close of one hour, the insensible loss had not only reached, but had even fallen below the normal standard, ·79; while in the second, in which the loss was greater, it had not quite reached the standard. From the variations in the amount of insensible perspiration caused by exercise we now pass to the consideration of

The Relations of the Insensible and Sensible Losses to each Other.

Among those who have devoted time and study to this subject, no one is more worthy of mention than the philosopher Sanctorio. In his work "Medicina Statica," published in 1720, the reader will find many hard-earned facts, which would be considered ornaments to the pages of any of our modern scientific journals; and though he had to contend with numerous difficulties and his writings bear the impress of the times in which he lived, yet his work was a grand step in the right direction, for he made the attempt to bring the great questions of physiology to the test of the balance, and force from nature truthful but unwilling answers.

From the Aphorisms of Sanctorio we extract the following, in order to demonstrate how highly he esteemed the subject with which we are dealing in this paper.

"*Aphorism* 2.

"If a physician who has the care of another's health is acquainted only with the sensible supplies and evacuations, and knows nothing of the waste that is daily incurred by insensible perspiration, he will only deceive his patient, and never cure him."

In order to enforce the truth and importance of the above, he gives the results of his experiments, obtained by the balance:

"*Aphorism* 6.

"If eight pounds of meat and drink are taken in one day,

the quantity that usually goes off by insensible perspiration in that time is five pounds."

In evidence of the close and earnest manner in which Sanctorio examined the various phenomena connected with our subject, I quote the following:

"*Aphorism* 56.

"The body does not perspire at all times alike, for in the first five hours after eating there wastes about a pound, the next seven hours about three pounds, and from the twelfth to the sixteenth (at which time there will be need of a fresh supply) nearly half a pound."

"*Aphorism* 65.

"Even those men who are in a perfect state of health, and observe the utmost moderation in living, once in a month increase beyond their usual weight to the quantity of one or two pounds, and at the month's end return again to their usual standard, in the same manner as women do, but then by a critical discharge of urine, it being either increased in its quantity or more turbid."

The above bear the impress of the mind that originated them; and though Sanctorio has in his book added many other aphorisms which we now know must have been purely ideal, still these and many others, which we have neither time nor space to quote, give evidence of deep thought; and when we take into consideration the times in which he wrote, we can almost forget the mixture of ideal fancies with experimental results.

Passing to the consideration of the relations of the insensible to the sensible losses, and taking the period of sleep as presenting conditions which are free from external influences, we find that the insensible loss by the lungs and skin is greater than the sensible loss by the kidneys, as is shown in the following table:

Insensible loss per minute	·47 gramme,	Temp. 50°	Dew Point, 42°
Sensible " " "	·40 "	" "	" " "

Not only is the insensible loss greater than the sensible by the kidneys during the night, but the same relation exists

throughout the twenty-four hours, as we shall find if we compare together the total diurnal amount of these excretions.

Before giving the results of my own experiments on this subject, I shall recapitulate briefly what has been done by others who have made these excretions a subject of study.

Sanctorio, as we have just seen, states that if the ingesta in one day amount to eight pounds, no less than five pounds find their way out of the system in the form of insensible perspiration.

Seguin made the attempt to determine the loss through the skin, by inclosing himself in an air-tight bag, and ascertaining the rate of loss with the bag and without it, the difference representing the loss through the skin, which he found was 2·75 pounds per diem. He also found that the loss by the lungs being 7, that by the skin was 11.

Valentin obtained, as the result of his experiments, a loss of 2·35 pounds per diem by the skin.

Many objections may be urged against experiments performed in the manner in which those of Seguin were conducted. Inclosing the body in an air-tight membrane interferes seriously with the functions of the skin, and the results obtained by experiments, conducted under conditions which alter or interfere with the normal state of the body, are not to be relied on unless they are sustained by others which give similar results, and against which such objections cannot be advanced. Seguin, inclosed in his air-tight bag, must really have obtained results which represented the body while in a vapor-bath at over 90° Fah. which is not by any means its normal condition. And not only was the temperature and state of moisture of the air changed, but the escape of the various excreta interfered with, and consequently the rate of loss changed from the normal standard.

In my experiments, all such sources of error have been carefully avoided; the body has always been in the normal condition—during the day employed in the usual avocations, at night at rest in bed.

In the spring of 1856, I made several experiments on my own person, in order to determine the loss by insensible perspiration through the respiratory apparatus. I have thought

that it would add to the interest of this portion of our subject if they were grouped with those on perspiration through the skin and the sensible evacuations, viz. fæces and urine, and compared with the ingesta which have also been subjected to examination. Carrying out this idea, we will first recapitulate the results of the experiments on respiration, and then pass to the consideration of the amount of liquid and solid food introduced into the system each day.

The great differences existing in the estimation of the amount of material voided from the lungs being due, to a considerable extent, to the method employed in obtaining the results, which was, by forcing the expired air through water, and collecting it in jars or gas-holders, I endeavored to arrive at a more reliable conclusion by avoiding as far as possible the chief cause of error, which was the pressure of the column of water through which the expired air had been forced in order to collect it in the jars.

This was accomplished by conducting the expired air into a metallic condenser kept at a fixed temperature (32° Fah.). During its passage through the condenser the air deposited the moisture it contained, and from the increase in the weight of the apparatus at the close of an experiment, the rate of the loss was determined. Or, if it was desired, the dew point of the breath and that of the atmospheric air being known, the quantity of air passing through the instrument from the lungs could be calculated. The trials were always continued for twenty minutes, so as to avoid error from slight variations in the rate of breathing.

The average of many experiments showed that the number of respirations per minute being 16, and the dew point of the breath 94°, the volume of each respiration was 38·8 cubic inches, and the diurnal insensible loss through this channel 464 grammes. These results were published in the New York *Medical Times*, for July, 1856.

The loss by the skin was obtained by deducting the loss by the lungs (464 grammes) given above, from the total loss by insensible perspiration, 1108—which gave as loss by the skin alone 644 grammes.

The loss by urine for my own person I determined, and

published in my thesis many years ago. A few experiments were made in order to find whether it had undergone any change; as it was materially the same as then obtained, I discontinued my experiments and adopted the old standard, which was 1106 grammes per diem.

The amount of fæces passed was also determined by weighing before and after the discharge. The average per diem quantity was found to be 159 grammes.

In the experiments made on the ingesta, care was taken not to eat or drink anything except at the regular meals. Then by weighing before and after eating, and making due allowance for the insensible loss during each meal, the quantity consumed was accurately determined.

The average of results, extending over a period of more than a month, gave the following quantities: Breakfast, 386; Lunch, 380; Dinner, 1231; Tea, 376. Total per diem 2373 grammes.

Collecting these results together we obtain the following table which shows at a glance the diurnal amount of sensible ingesta, and the manner in which the food and water are finally disposed of by the system.

Ingesta.		Egesta by		
Breakfast,	386	Skin,	644	Insensible 1108.
Lunch,	380	Lungs,	464	
Dinner,	1231	Urine,	1106	
Tea,	376	Fæces,	159	
	2373 grammes.		2373 grammes.	

Converting the above into Troy pounds, we find that the diurnal amount of food and liquid taken is about 6.36 pounds, of which 2·97 pounds are lost through the skin and lungs, and constitute the insensible perspiration of Sanctorio. It will be remembered that the statement he made was, that out of eight pounds of food and liquid taken into the system, five pounds pass off as insensible loss. Though the above results do not confirm his figures, they support and bring prominently forward the great importance of the losses through this channel, and the necessity of devoting more attention to the study, not only of the amount but also of the composition of the insensible losses, before we can clearly comprehend the physiological operations and pathological changes in the human system.

In the following table the results of my experiments are compared with those of others who have made the excretions of the body the subject of examination and study:

	Ingesta.	Loss by Skin.	Loss by Lungs.	Total insensible loss.
Sanctorio,	8 lbs.			5 lbs.
Seguin,		2·75 lbs.	1·75 lbs.	4·50 lbs.
Valentin,		2·35 lbs.		
Krause,		2·15 lbs.		
Draper,	6·36	1·72 lbs.	1·25 lbs.	2·97 lbs.

On submitting the above to examination, it will be found that my results agree with those of Seguin in confirming the statement that the loss by the skin is greater than that by the lungs, though the proportion was not so much in favor of the skin as it was in his experiments. The difference is probably due to the manner in which his results were obtained, viz. by the use, as before stated, of an air-tight bag, which must have stimulated the action of the skin by cutting off the loss of heat through evaporation, and so by congestion elevating the temperature of the body, and thereby increasing the insensible loss, just as when we take a vapor-bath the loss is greatly increased, and perspiration in the sensible form of sweat literally pours off the body.

Effect of Ingestion of Food on Insensible Perspiration.

In the examination of this subject, we shall first take up the consideration of the effect on the nocturnal loss of taking food in larger quantities and later in the evening than is customary, and then show the difference in rate of loss before and after meals.

Variation in the regular time of taking the last meal, or change in the character of the food, impresses a change on the nocturnal loss, as is demonstrated by the fact that on one occasion, after a late hearty dinner, it rose to ·77 gramme per minute, and in two other instances, when a light supper was taken, to ·55, the average being ·47. From this we conclude that the ingestion of food causes an increase in the rate of insensible loss.

If we examine the rate of loss before and after a meal, the body being kept in a state of rest, we obtain evidence in support of the above statement. During the month of January, my experiments were so arranged as to give the rate of loss in

a state of rest before and after dinner. The averages of the results obtained show a difference of ·02 gramme. The rate before dinner being ·79, while that after dinner was ·81.

With Sanctorio, I therefore agree that the taking of food influences the rate of loss by insensible perspiration, but the increase in loss after a meal, as obtained in my experiments, is far less than that obtained by him.

Variations in the Weight of the Body from Day to Day during extended Periods.

Both Gall and Levy hold the opinion that in men there are periodical manifestations similar to those existing in females, which are scarcely visible in robust individuals, but are often very evident in those of feeble constitutions, or in persons who are suffering from fatigue following hardship. Hammond also states that he has noticed the same variations in many of his friends, the monthly manifestations being in the form of headache, epistaxis, diarrhœa, etc.

In order to arrive at a just conclusion regarding this portion of our subject, I extended my observations on the weight of the body over a period of three months, so as to obtain sufficient data from which some reliable statement could be deduced.

The greatest care was taken to weigh as nearly as possible at the same hour of the day in the different experiments. The time chosen was half-past seven A.M., that is, shortly after rising in the morning. All irregularity regarding the evacuations was scrupulously avoided. The results are given in the following table, together with the temperature and dew point. The average weight for each month is also stated.

April, 1863.

Date.	Weight.	Temperature.	D. Point.	Date.	Weight.	Temperature.	D. Point.
18	64·155			24	63·710		
19	64·145			25	63·417		
20	63·520			26	63·790		
21	63 475			27	63·713		
22	63·693			28	63 615		
23	63·375			30	63·895		

Average Weight during April, 63·709 grammes.

May, 1863.

Date.	Weight.	Temperature.	D. Point.	Date.	Weight.	Temperature.	D. Point.
1	64·885			18	65·345		
3	64.007			19	65·770		
4	64·505			20	65·620		
5	65·060			21	64·420		
6	65·055			22	64·870	67	65
7	64·860			23	64·545	75	68
9	64·884			24	64·710	67	62
10	65·200			25	64·520	62	56
11	65.520			26	64.550	60	56
12	65·070			28	65·020	64	60
13	65·300			29	64·920	70	65
14	65·285			30	64·960	73	70
15	65 220			31	64·880	73	70
17	65·320						

Average Temperature and Dew Point, 68°, 64°.
Average Weight during May, 64·975.

June, 1863.

Date.	Weight.	Temperature.	D. Point.	Date.	Weight.	Temperature.	D. Point.
1	64·600	71	67	16	64·160		
2	64·300	67	60	17	64·745		
3	64·920	66	60	18	64·900		
4	65·110	65	60	19	64.600		
5	65·190	64	58	20	64·550		
6	65·120	62	59	22	65·320		
7	64·990	63	57	23	65·320		
8	64·870	64	59	24	65·095	65	60
9	64·770	66	60	26	65·820	67	63
11	64·690	71	66	27	65·345	65	60
12	65·180	70	67	28	66·120	67	63
13	65·250	67	64	29	66·870	67	64
14	65·230	65	60	30	66·290	68	65
15	64·685	70	66				

Average Temperature and Dew Point, 66°, 61°.
Average Weight during June, 65·112.

July, 1863.

Date.	Weight.	Temperature.	D. Point.	Date.	Weight.	Temperature.	D. Point.
1	65·190	71	67	11	65·390	75	75
2	66·220	73	70	13	66·190	72	71
3	66·350	73	67	15	65·650	70	67
4	65·970	73	72	16	65·470	75	73
5	65·510	70	66	18	65·695	70	67
6	65·920	71	68	19	65·685		
7	66.320	71	67	21	65·205	72	70
8	65·345	75	72	22	65·520	67	65
9	65·250	74	70				

Average Temperature and Dew Point, 72°, 69°.
Average Weight during June, 65·755.

From these experiments, we find that though there is at times an increase in weight, and variations from day to day also exist, they do not appear to be reducible to any special order, or referrible to any particular cause, but are irregular, and do not demonstrate the law which Sanctorio gives in his 65th Aphorism—that in men in perfect health there is a monthly increase in weight, and also a diminution marked by a critical discharge. They do not even establish with complete satisfaction the extent to which changes in the dew point affect the weight of the body. The only fact that is satisfactorily shown is, that during the period of the experiments, viz. from April 15th to July 22d, there is an increase of weight each month, which is probably due to the decrease in physical exercise during the hot weather.

Reviewing the results we have obtained, we conclude—

1st, That the effete materials arising during the disintegration of muscular tissue, are evacuated as insensible perspiration, and not as urea through the urinary apparatus, a definite time being required after long continued exercise before the insensible losses regain the normal standard.

2d, That the loss by the skin and lungs is greater than that by the kidneys.

3d, That we cannot establish the existence of a regular monthly increase in weight, as supposed by Sanctorio and others.

ON OVARIOTOMY.

By E. R. PEASLEE, M.D., LL.D.

READ BEFORE THE ACADEMY, JUNE 1, 1864.

Mr. President and Fellows:

It is not my purpose at the present time to give the details of my own experience in Ovariotomy. My cases, only six in number, have all proved successful, and may all be found reported in the American Journal of the Medical Sciences.*

In a paper read before this body a few weeks since,† I arranged the various surgical methods of treatment of ovarian tumors under the following heads:

A. Simple Tapping.
   1. Per parietes abdominales.
   2. Per vaginam.
   3. Per rectum.

B. Tapping followed by pressure.

C. Tapping and formation of permanent opening in sac.
   1. Externally.
   2. Per vaginam.
   3. Per rectum.
   4. Internally.

D. Tapping followed by injections of iodine.

E. Ovariotomy, or extirpation of the ovarian mass.

I at that time considered all the preceding methods of treatment excepting ovariotomy, and deduced certain conclusions which I shall presently repeat here. This evening I propose to consider the value and the justifiability of ovariotomy as a

* April, 1851; Jan. 1856; Oct. 1858; April, 1863; July, 1864.

† March 17, 1864.

surgical procedure, as well as the circumstances, and the methods of effecting the various steps of the operation, which promise the greatest success.

There is, however, a single point to which I desire to direct attention before I enter upon the topics just mentioned, viz. the assertion of English writers that ovariotomy is an operation of British origin. And the following statement of facts will show how much foundation there is for such a claim.

It is generally asserted in England that the operation of ovariotomy was first suggested by Wm. Hunter one hundred years ago;* and that the suggestion was endorsed by John Bell, afterwards Prof. of Surgery at Edinburgh. This statement perhaps needs confirmation; but at any rate Dr. Ephraim McDowell of Kentucky was the first who ever performed the operation; though, as he had been a pupil of John Bell, he might have heard it suggested as possible by his teacher. Dr. McDowell performed his first operation (which was successful) in 1809, or fifty-five years ago; and up to the time of his death, in 1830, he had operated thirteen times, and is known to have succeeded in at least eight of his cases.

Dr. McDowell sent a report of some of his first operations to his former teacher, Prof. Bell; but the latter having died before it reached him, it fell into the hands of his successor, Mr. Lizars of Edinburgh. The operation was, however, never performed by any one except Dr. McDowell for fourteen years after his first attempt. In 1823 Mr. Lizars attempted it but failed, inasmuch as there was no ovarian tumor to be removed; nothing but "tympanitis and obesity." During the next two years he operated twice more; once successfully, and once without completing the operation, on account of another mistake in diagnosis.† Dr. Granville, of London, attempted ovariotomy twice in 1827. One case proved to be a uterine tumor, and the other was abandoned on account of adhesions.

There were no more operations in Great Britain for nine years, or till 1836. Dr. McDowell died in 1830, and at that time the statistics stand thus: Dr. McDowell had operated

* In 1762; and he rather discouraged it.

† All three of Mr. Lizars's cases recovered, however.

thirteen times, and at least eight times successfully; all the attempts at operations in Great Britain had been five, of which three only had been completed, and two in all had been successful. This was also the state of things in Great Britain up to 1836; though in the meantime the operation had frequently been successfully performed in this country, and twice at least in this city by Dr. Rogers and Dr. Billington.

If ovariotomy is of British origin, then, Wm. Hunter originated it; which he certainly did not. He simply suggested its possible practicability, which suggestion was probably repeated by John Bell; but it had been before the profession in Great Britain for forty-seven years to no purpose; and, for aught any one can show or perceive, might have remained so a century longer, had not an American surgeon reduced the suggestion to practice. It is an American operation, suggested by a Scotch intellect. Scotland, however, seems to have disowned it almost entirely; for since the operations of Mr. Lizars, it has very seldom been attempted; and up to Sept. 1862, it had succeeded only in a single instance.* In Ireland also the operation had been performed but three times up to April, 1862, and always with a fatal result.† Up to the present time, indeed, ovariotomy has not found favor in any country in Europe excepting England, and to some extent also in Germany. In the latter, however, it is not yet attended with much success; while in France it is still denounced by the profession generally, and very seldom attempted.

It is not uninteresting, in connexion with the question as to its origin, to show‡ that this operation made but a very slow progress into favor even with English surgeons till within the last eight or ten years; although it had meanwhile been so often successfully performed in this country.

Recurring to the year 1836, when only one successful operation had been performed in England, we find that three operations, all successful, were that year performed. In 1838 there was one successful operation; in 1839, one successful, and one

* London Lancet, Jan. '63, page 70.

† Amer. Journal of Med. Sciences, Jan. 1863, p. 239.

‡ T. Spencer Wells's History of Ovariotomy in Great Britain; in Medico-Chirurgical Transactions, Vol. xlvi., 1863.

not completed. In this year the operation was also first attempted (but not completed) in a London hospital; and in 1840 it was first completed in a London hospital by Benjamin Phillips, though the patient died. The first successful operation in a metropolitan hospital was performed by Cæsar Hawkins in 1846; and the next successful one did not occur till twelve years after this one. Dr. Clay, of Manchester, commenced his career in 1842, saving three out of four patients. Aston Key first removed both ovaries in 1843; the patient died on the fourth day; and Bransby Cooper had a similar result the same year.

Up to 1842 the operation had never succeeded in London, though there had been ten successful cases in the Provinces. In November of this year Mr. Walne had the first successful case in the city. He had two fortunate cases in 1843. In the next three years (to 1846) there were eleven operations in England, almost all successful. I find no more mentioned till 1849 and 1850; two successful cases each year. Mr. Duffie first operated, leaving the pedicle outside, in 1850. I. Baker Brown operated nine times from 1852 to 1856, saving but two patients; and then ceased for more than four years. Besides his cases, I find but nine operations in England in the seven years from 1850 to 1857. T. Spencer Wells commenced his career as an ovariotomist in 1857; and since that time the operation has been frequently performed in England. Mr. Hutchinson first operated (successfully in two cases) in 1858; and he first used the clamp in the management of the pedicle of the ovarian tumor.

It is therefore only in the last six or seven, and especially in the last four or five years, that ovariotomy has frequently been resorted to in England. Great improvements have also been made in the manner of operating during this short period; and for these we are mainly indebted to English operators.

Up to the commencement of the present year, about six hundred cases of ovariotomy had been reported in the British possessions, this country, and Germany—a sufficient amount of experience, it would seem, to determine the merits, both absolute and relative, of the operation, and the best method of performing it. Still, there is hardly a question connected with the

subject which is actually settled by the profession, even in this country.

In deciding, so far as I may, the points just alluded to, I shall discuss the subject under four distinct heads or inquiries:

I. Should ovariotomy be recognised as a legitimate operation in surgery?

II. In what classes of cases, and in what special circumstances, is it proper to resort to it? and what conditions forbid it?

III. How should the operation be performed?

IV. What is the appropriate treatment after ovariotomy?

I. SHOULD OVARIOTOMY BE RECOGNISED AS A LEGITIMATE OPERATION IN SURGERY?

Those who maintain that ovariotomy is never justifiable, base their objections upon three distinct grounds:

1. On statements sustained by mere *à priori* reasoning.
2. On authority.
3. On the asserted unreliability of the statistics of ovariotomy.

1. It ought to be unnecessary to remark, that no practical question in our art can ever be settled by either *à priori* reasoning or by mere authority. For, while either is deciding that a thing is impracticable, some one may do the thing in question. Dr. Lardner demonstrated by that kind of reasoning that no steamer could ever cross the Atlantic, and had hardly stated his reasons at length before the thing deemed impossible was an accomplished fact. Many similar instances have occurred in the history of our profession. I will merely repeat the objections based on *à priori* considerations to which I have alluded; but they have been so ably answered and refuted by Prof. Miller of Louisville,* and Prof. Simpson of Edinburgh,† that I will not further occupy the time of the Academy with them:

1. The great danger of the operation.

* American Journal of Med. Sciences, April, 1859, p. 336.

† Lectures on Diseases of Women.

2. So violent a remedy not sanctioned by the nature of the disease. It may terminate spontaneously.
3. Palliating treatment may prolong life indefinitely.
4. Even if the operation succeeds, it may not secure permanent relief.
5. The difficulty of a correct diagnosis.

Perhaps, however, the fourth objection needs some explanation. It means that, if you operate and cure the patient, the other ovary may possibly become diseased at some future time!! The preceding objections apply as truly to most other capital operations. The last objection alone has any special weight as applied to ovariotomy; but the difficulty of diagnosis really existing in some cases, may be practically neutralized in perhaps every instance by the two following rules:

1*st*. *Never regard ovariotomy as an operation to be performed in a hurry, or while the patient is in good health.* If you wait some months, or a year or two, if possible, you will gain time to form a correct opinion of the case, and if tapping becomes necessary, the diagnosis can thus be confirmed or corrected; while at the same time the chances of recovery from ovariotomy are on the whole, perhaps, by that palliative operation increased.

2*d*. *When you commence the operation of ovariotomy, always regard the incision as merely explorative, until you reach the point to determine whether you will finish the operation or not.*

I should, however, add that most of the blunders in diagnosis which have been quoted to the discredit of ovariotomy, were committed before the invention of the uterine sound, or by those who refuse to use it—an instrument which I consider indispensable to a decisive diagnosis in cases of ovarian tumors. At any rate, the difficulties of diagnosis have been generally overcome by recent operators. Dr. Savage, of London, states* that in fifty-two cases, T. Spencer Wells committed not a single mistake; and Dr. Tyler Smith, in fourteen cases, committed but one error in diagnosis.† Still, there are two points which can in no case be precisely determined till after the operation is

* Amer. Journal of Med. Sciences, Jan. 1863, p. 235.

† See T. S. Wells's remarks on the difficulty of diagnosis. Amer. Journal of Med. Sciences, Jan. 1863, p. 233.

commenced—viz. the side on which the disease originated, and the extent of adhesions. The former is, however, a matter of no practical importance; and the latter will, in but a very small proportion of the cases, deter us from completing the operation.

The third objection mentioned to the operation suggests the inquiry—What is the average duration of the disease, after the tumor becomes appreciable, before the fatal termination? Velpeau, who is opposed to ovariotomy, assigns a period of four, six, or even twelve years; while Mr. Thomas Lee gives the average as only two years, though some of his cases lived four years. Cazeaux states that of thirty-one cases, seven only lived over ten years.* In my own observation the average duration has not exceeded four years. But the progress is more rapid if the patient is quite young (less than twenty to twenty-two), and the reverse if over fifty. I have found polycystic tumors more rapidly fatal than the monocystic, since they cannot be so completely relieved by tapping; and the solid tumors the slowest of growth. Flaccid cysts also grow very slowly. Cruveilhier saw one already thirty years old. But even these cases of long duration of the disease furnish no ground against ever performing the operation of ovariotomy; but only against operating too early.

2. Nor can mere *authority* decide *a practical* question, whether of individuals or of learned societies. The first question that always occurs is—What special qualifications have these persons or this body to decide the question? A State Medical Society, a few years since, denounced the operation I am considering by a formal vote. But what surgeon, even among the members of that association, was in the least influenced thereby? If the self-constituted authority has practical experience or observation, it has a special right to be heard; if not, it has no such special right or influence. It is therefore our duty to challenge, and often to reëxamine, the conclusions in our science which are based on mere authority or the general sentiment of the profession, as well as on *à priori* considerations.

* The remainder averaged only two to two and a half years. Amer. Journal of Med. Sciences, April, 1859, p. 336.

I will rapidly pass in review some of the authorities opposed to the operation of ovariotomy; the most distinguished being the late Prof. Mütter of Philadelphia, Mr. Liston, Dr. J. M. Duncan of Edinburgh, Dr. Robert Lee of London, and several members of the Imperial Academy of Medicine of Paris. It is true that enough has been done since these gentlemen promulgated their views, to establish the claims of ovariotomy to be accepted as a legitimate operation; but as they are still quoted by its opponents, they should receive some notice here.

Now it occurs to remark at the outset, that not a single one of the gentlemen I have just mentioned ever performed the operation of ovariotomy; most of them never saw it performed; and one, for a long time after he had committed himself against the operation, refused to see it performed. Besides, a majority of them are mere physicians who never engage in surgery at all. By what special right, therefore, do they assume to decide this question? Is not the opinion of Dr. Atlee alone, or of Dr. Clay, of Manchester, worth more than any number of opinions from such a source? These two gentlemen are entitled to speak, having performed the operation over one hundred times each. But to return.

Prof. Mütter adopted the ideas upon ovariotomy of Mr. Liston, whose volume on Operative Surgery he edited; and like his original, he "set his face against the operation, and thought he always should." His objections were some of the *à priori* considerations I have already quoted. Dr. J. Matthews Duncan,* a physician, admits that cases justifying the operation may possibly occur; but asserts that there is no class of cases for which it is a scientific therapeutic measure. I shall expect to define that precise class of cases further on. He refers "all such difficult and complicated practical questions as this to the arbitrament of professional opinion, as the ultimate resort;" and adds that "this opinion is, generally speaking, decidedly against the propriety of ovariotomy."

But how does professional opinion always decide every new question at first? In the negative, of course, as is shown by the history of every improvement in our art. The profession are

* London Lancet, May, 1857, p. 519.

not at once competent to decide affirmatively, and must therefore decide negatively, if at all. The burden of proof lies on the innovator, not on the profession; and until that proof is given, they are silently indifferent, or inactively opposed to the innovation. So much for the value of the arbitrament of professional opinion in "such difficult question as this." But in all such instances, some few individuals will exert themselves as *active* opponents of the novelty. These are the *champions of the negative proposition*, and it is interesting to see them always going over the same ground. They first attempt to prove that the thing proposed is impracticable; driven from this proposition, they next try to show, by garbled quotations from Hippocrates and Galen, that it was done more than two thousand years ago; and when the profession are at last obliged to admit the merits of the new procedure, they make a last effort to demonstrate that it is of no account after all. It is therefore no proof that an operation is not justifiable, that the opinion of the profession is at first, "generally speaking, decidedly opposed to it." And if it be opposed to ovariotomy up to the present time, I hope that opinion may be modified by the facts hereinafter adduced.

Dr. Robert Lee is merely an obstetrician, but not an obstetric surgeon, and therefore has no special claim to be heard on this question. He, however, denounces the published statistics of ovariotomy as worthless, since all the cases have not been reported; he himself having, after long research, found thirteen unsuccessful cases which had never been reported. As an offset to this idea, I would remark that I have found over fifty successful cases which have never been reported.* Dr. Lee thinks it "unphilosophical to set aside the experience of the world during a long course of years, and now to substitute in its place the experience and the marvellous success of a few practitioners during the last two years." But it occurs to us that the experience of the world has not been very extensive in *ovariotomy* during a very long course of years, while the improvements of the last two years before Dr. Lee expressed this opinion, were of the greatest importance. He doubts if

* Thirteen cases by Dr. Kimball of Lowell, and forty by Dr. W. L. Atlee of Philadelphia.

human life has been prolonged by the operation, when we come to offset those who have been killed by it against those with whom it has succeeded. Still, he concludes by admitting that ovariotomy may *sometimes* be desirable, though it is unjustifiable when the life of the patient is not in immediate danger, and when there is not a great probability that the life of the patient will be saved by the removal of the disease.* The precise modifications this last proposition should receive, will be made under the next head. I only add here, as indicative of the *animus* of Dr. Lee in connexion with this subject, that, though repeatedly invited to witness the operation of ovariotomy, and having often appointed to do so, he would always fail, till the 17th of November, 1862—a day thus rendered memorable in the history of ovariotomy—when he actually witnessed an operation by T. Spencer Wells. This operation was successful, but it produced, it would seem, a very peculiar effect upon Dr. Lee. Instead of watching its steps with interest, and discussing in his own mind the chances for a successful issue, as we would suppose so laborious a searcher for truth in connexion with this subject would have done, he says, in speaking afterwards of the occasion before a learned society: "I thought of Judas Iscariot," and then quoted and endorsed the coarse expression of Liston, who used to call ovariotomists "belly-rippers, with a B before and a B behind."†

The discussion‡ upon ovarian cysts, in the Imperial Academy of Medicine of Paris, was commenced in October, 1856, and continued till the next February, and the following members, half of them eminent surgeons, participated in the debate: Velpeau, Cruveilhier, Cloquet, Jobert (de Lamballe), Malgaigne, Huguier, Guerin, Gimelle, Trousseau, Piorry, Moreau, Robert, Barth, and Cazeaux. With a single exception, all these gentlemen condemned ovariotomy as a rash and unjustifiable procedure. M. Malgaigne said "the statistics of ovariotomy prove nothing; we all know the value of statistics where all the successes are collected, and all the reverses concealed." Piorry, a

* London Lancet, May, 1863, p. 339.

† London Lancet, Feb., 1863, p. 139.

‡ Reported in the Bulletin de l'Academie Imperiale, for Oct. 1856 to Feb. 1857.

physician, admits that "in certain circumstances we might attempt the excision of ovarian tumors, but to do this we must possess an American audacity" (une audace Americaine). But Cruveilhier's ideas were most remarkable, and have been the most frequently quoted by the opponents of ovariotomy. After stating that two kinds of ovarian tumors (the solid and the polycystic) "are marked by the seal of incurability, there being no treatment for them, either palliative or curative," and admitting that even these tumors "have a great many times been removed with success, especially in England and America," he still adds, "I do not think this bold operation entitled to be cited in science." Eminent as these gentlemen are in their respective spheres, I have already suggested the reason why they can claim no special right to decide the question under consideration. I may also add that the invidious accusation of Malgaigne is not sustained by facts; while the conclusion of Cruveilhier is inconsistent with his own admission as to the success of the operation. Such ideas may perhaps answer still, though seven years old, for Paris and France; but not for a latitude where ovariotomy in its improved condition is understood.

A single one, however, of the participants in that discussion had a special right to speak with authority on this subject. His studies and his constant acquaintance in practice with the nature and progess of ovarian tumors, qualified him to hold an intelligent opinion on this subject. I allude to the distinguished surgeon-accoucheur, Cazeaux, and whose voice alone was raised in favor of the operation. If I say his opinion alone was worth more than all the other opinions, the grounds of such an assertion have already been given. It is also a pertinent fact in this connexion that not a single member of the Obstetrical Society of London has raised an objection to the principle of ovariotomy; though several of them were for years opposed to it (among them Drs. Hall Davis and Tyler Smith) until they became acquainted with the facts.

In contrast with their French *confrères*, the leading English surgeons have made no opposition to ovariotomy; but the reverse, even, so far as they understood the subject. "With

the exception," says Mr. Charles Hawkins,* "of those who have made this class of disease a specialty, not three of the leading surgeons of London have said anything for or against this operation." And Mr. Curling adds, the reason of this is, that "hospital surgeons can speak only *from a limited experience.*" Of late, however, Mr. Fergusson, Mr. Erichsen, and other Hospital surgeons have frequently endorsed the operation in the most effectual way, by performing it themselves.

In closing my remarks on the value of mere authority in deciding a practical question, I wish to record my protest against borrowing our medical and surgical opinions from any foreign source. We should be thankful for facts from any and every source; but I trust we are capable of forming our own opinions upon the facts. To go to France, where ovariotomy is almost never performed, or to Germany, where seventy-four out of one hundred operated upon, die, to inquire if it be right for us in America to operate, is absurd. In this case it is *we* who have the facts.

3. I next consider the bearing upon the question under consideration of

The Statistics of Ovariotomy.

The question whether ovariotomy is justifiable, can be finally settled only by an appeal to its statistics; and if we assume with Dr. Robert Lee and M. Malgaigne that the latter are totally unreliable, its decision is impossible. I have already exposed the shallowness of Dr. Lee's objections, and now add that Malgaigne's assertion, that "ovariotomists collect all the successes and conceal all the reverses," is a gross libel on that class of operators; the fact being that all the experienced ovariotomists have reported *all* their cases, unsuccessful and successful, when they make a report at all. And this is as good as we can have if we would collect the statistics of any other capital operation. The recent statistics of ovariotomy are therefore as reliable as those of any other operation.

In two respects, however, former statistics of ovariotomy should undergo revision.

1. The grouping together of complete and incomplete opera-

* London Lancet, June, 1863, pp. 340 and 528.

tions of ovariotomy in the same collection, has no foundation in reason or justice. An incomplete or unfinished operation is not an operation of ovariotomy at all. The latter operation implies the removal of the diseased ovary, after bringing it into view by opening into the abdominal cavity.* If we only do the latter, we have performed gastrotomy merely, but not ovariotomy. Now, mere gastrotomy is comparatively not a dangerous operation; gastrotomy, with removal of the ovarian mass, or *ovariotomy*, is a dangerous operation. Is it just to the latter to class it with another far safer operation, when we come to speak of its fatality? If I perform one hundred operations of ovariotomy, and fifty cases recover; and fifty operations of gastrotomy, and all recover, and then arrange them under one head —ovariotomy, complete and incomplete—I thus have one hundred and fifty cases of ovariotomy, and one hundred recoveries, or about 67 per cent.; while in fact I had but fifty recoveries out of one hundred actual cases, or fifty per cent. instead of sixty-seven. I shall therefore, in my own statistics, include only actual cases of ovariotomy, and quote the former statistics only after being thus corrected.

2. As a matter of taste, I shall always state the number and the per cent. of those who have been *cured* by the operation; former statisticians having told us how many have died after it.

I first call attention to the statistics of Dr. G. N. Lyman of Boston, published in 1856.† Of the three hundred cases mentioned by him, two hundred and twelve were cases of ovariotomy; and of these 57.22 per cent. recovered.

Mr. John Clay of Birmingham, England, brought the statistics of ovariotomy up to the beginning of the year 1860. Of his five hundred and seventy-seven cases, four hundred and twenty-five were cases of ovariotomy; of whom two hundred and forty-two, or fifty-seven per cent., recovered. The countries in which three hundred and eighty-six of these operations were performed, and their results, were as follows:

* I have not devoted any time in this paper to *partial* ovariotomy, or excision of a portion of the sac, since Mr. Clay of Birmingham shows that this operation is far more dangerous than the excision of the entire tumor; fourteen dying out of twenty-four.

† Amer. Journal of Med. Sciences, April, 1857, p. 462.

Great Britain,	222 cases;	and 127—57.2	per cent.	recovered.
United States,	113 "	64—56.63	"	
Germany,	51 "	13—25.5	"	

Dr. Fock of Berlin collected two hundred and ninety-two cases of ovariotomy; of whom one hundred and seventy, or fifty-nine per cent., recovered.*

Of forty-four cases of ovariotomy collected by Simon, they all having occurred in Germany, only twelve, or twenty-seven per cent., recovered. Dr. Simpson also mentions a collection of one hundred and seventy-nine cases by Dr. Atlee; of whom one hundred and twenty, or sixty-seven per cent., recovered.†

If we omit the extremes just given (the very unfavorable results of ovariotomy in Germany, and those also of Dr. Atlee's collection) we find the average per cent. of Drs. Lyman, Clay, and Fock to be 57.74 per cent. saved by the operation. Can the gentlemen who denounce the operation show any better results than these from their *do-nothing* system?

But the preceding statistics are relatively unfair to ovariotomy, since they comprise *all* the operations reported, since the very first by Dr. McDowell. This is not the case with the statistics of any other capital operation. It is admitted that they must all go through a formative stage at first. Nobody goes back to Frère Come to commence the statistics of lithotomy, nor to John Hunter to begin with those of the ligation of arteries for the cure of aneurisms. I have therefore confined my own statistics to cases which have occurred during the last four years, viz. 1860–61–62–63. I have thus endeavored to include all the cases which were omitted by Mr. Clay, and which have occurred since his collection was published.

Rejecting a very few cases which were reported so soon after the operation as to leave some doubt as to the final result, I have collected one hundred and fifty cases of ovariotomy‡ not before collated; of whom nine-nine, or sixty-six per cent, recovered. Dr. Simpson has shown that even the average before mentioned is more favorable to ovariotomy than

* Prof. Simpson's Lectures, p. 379.

† Lectures, p. 379.

‡ To be published in Amer. Journal of Med. Sciences, Jan. 1865.

are the statistics of other capital operations to them; and I refer to his Lectures on Ovariotomy for the particulars. But even the result of my own collected cases, or the recovery of a small fraction less than two out of three of all operated upon, is still relatively unjust to ovariotomy; since very many of the one hundred and fifty operations were performed by inexperienced operators. In collecting the statistics of the other great operations, on the contrary, we select the practice of a single or of several hospitals; or at least we collate the results of experienced surgeons. But every one seems to feel at liberty to perform the operation of ovariotomy at least once, even though he never thought of performing any other important surgical operation; and the results of such temerity, of course, diminish the percentage of success which the experienced would obtain.

Let us then see what is the success of those who have had the most experience in ovariotomy.*

Dr. Clay of Manchester,	104	operations;	72	recovered.
T. Spencer Wells,†	55	"	37	"
Dr. Kimball (Lowell, Mass.),	23	"	15	"
I. B. Brown,	58	"	32	"
Dr. Dunlap (Ohio),	19	"	15	"
Dr. Tyler Smith,	14	"	11	"
	273	"	182	"

Here, then, we have an average of sixty-six and two-thirds per cent., or two out of three, recovering from ovariotomy, if we take the *whole* experience of the best ovariotomists. But if we only include their operations within the last four or five years, so far as known, and those performed under the most favorable circumstances, we get the following results:

* I regret that I cannot precisely state the success of Dr. Atlee of Philadelphia. He has not yet reported his last seventy cases; his whole number being now over one hundred.

† Up to Aug. 22, 1864, Mr. Wells had had one hundred and six operations and seventy recoveries.

T. Spencer Wells	last	15	cases;	14	recovered.
I. B. Brown	"	15	"	11	"
Dr. Tyler Smith	"	13	"	11	"
Dr. Dunlap	"	19	"	15	"

Here we have sixty-two operations and fifty-one recoveries; or eighty-two and one-quarter per cent. saved by the operation. I have no doubt the more recent experience of Drs. Atlee and Clay is equally successful, since they both state that their success is constantly increasing. And if we add to the above my own limited experience of six successful cases, the percentage of recoveries amounts to a small fraction short of eighty-four per cent. (83$\frac{1}{14}$.)

Tested statistically, therefore, on the same conditions as the other great operations are, ovariotomy now presents a success in this country and in England of at least seventy-five per cent. Can *any other* capital operation show such a record? And can any medical man cognizant of these facts continue to assert that ovariotomy is an unjustifiable operation, and one that should not be recognised in surgery, nor "cited in science?" Dr. Hall Davis of London for many years objected to it in his lectures, but now approves of it.* Mr. Fergusson now frequently performs the operation, though formerly prejudiced against it. Dr. Tyler Smith objected to it for twenty years, but is now, himself, one of the most successful of ovariotomists.† Dr. Savage, and the other authorities of the Samaritan Hospital, were opposed to it; but he is now its defender, "its success is so conclusive."‡ Dr. Charles West of London commends it in the last edition of his Lectures on Diseases of Women; having opposed it in the previous editions. How could these gentlemen conscientiously do otherwise than favor the operation, on knowing the facts. I conclude in the words of I. B. Brown: "Daily experience must satisfy every unprejudiced mind that this is more successful than any other great operation of surgery, and that all the old objections must give way."§

* London Lancet, June, 1862, page 391.

† Amer. Jour. of Med. Sciences, Jan. 1863, p. 233.

‡ Amer. Jour. of Med. Sciences, Jan. 1863, p. 235.

§ London Lancet, Sept. 1861, p. 183.

II. THE CLASS OF CASES TO WHICH OVARIOTOMY IS ADAPTED; AND THE SPECIAL CIRCUMSTANCES WHICH TEND TO A FAVORABLE RESULT, OR THE CONTRARY.

A. *To what Class of Cases is Ovariotomy adapted?*

The assertion of Dr. Duncan has already been quoted, that "though cases justifying the operation of ovariotomy may possibly occur, there is no *class* of cases for which it is a scientific therapeutical measure." I hope to show the incorrectness of this opinion.

I cannot discuss the pathological anatomy of ovarian tumors in this paper; and it will be understood that I also pretermit malignant diseases of the ovary, as not demanding any form of surgical interference. It is sufficient for my present purpose, as in my former paper, merely to recognise the two classes of ovarian tumors—the solid and the cystic—and the two varieties of the latter—the *monocystic* or unilocular, and the *polycystic* or multilocular. I also showed that for practical purposes we have the simple monocystic tumor on the one hand, while on the other, the polycystic and the solid tumor are to be grouped together. And having considered all the other surgical methods of cure except ovariotomy, I arrived at the following conclusions:

1. "Simple tapping of ovarian sacs is merely a *palliative* measure; by no means to be regarded as a harmless one in any circumstances, and proving fatal in one case out of seven (to four) when resorted to for the first time.

2. "All the *curative* methods I have considered, totally fail in cases of *polycystic* tumors, leaving *ovariotomy alone as adapted to them.* They all, moreover, in their application to monocystic tumors alone, give but a very slight promise of success, except the iodine injection, and, in a few cases, the tapping *per vaginam* and leaving the canula *in situ.* Besides, they are all, except the iodine injection and the formation of a permanent opening of the sac into the peritoneal cavity, as dangerous as ovariotomy, or even more so.

3. "Of all the curative methods I have considered, the iodine injection alone (and the tapping *per vaginam* and leaving

the canula *in situ*, in a few cases) is therefore to be commended, even in the treatment of monocystic tumors.

4. "Iodine injections are valuable as a curative method, if restricted to single sacs containing a clear, serous fluid, thus proving successful in one-third to one-half of the cases; they may, perhaps, succeed, if the contents of the sac are albuminous, provided the fluid is completely removed from the sac by injections of warm water before the iodine solution is used. But the latter failing in either case, ovariotomy alone remains as a curative measure.

5. "Injections of iodine may however *retard the refilling* of simple sacs, and even of one or more of the principal sacs of a polycystic tumor, and may be used with this expectation merely, when ovariotomy is out of the question.

6. "Iodine injections are not to be regarded as unattended by considerable risk; and which is probably much greater in a patient never before tapped.

7. "If in case of a monocystic tumor circumstances compel us to reject the treatment, with a curative intention, by the iodine injection, and by leaving the canula *in situ*, ovariotomy becomes the sole curative method in this case also, as well as in all cases of polycystic and solid tumors."

There are, then, two entire classes of ovarian tumors—the polycystic and the solid—which are, to quote Cruveilhier again, "marked with the seal of incurability." None of the methods before considered, nothing but extirpation, is of any avail with them. And ovariotomy has been performed with success in hundreds of such cases. Ovariotomy is therefore the *sole* and the scientific remedy in such cases.

It has, moreover, been shown that there are cases even of monocystic tumors also, which, after the ineffectual use of other curative methods, can be cured only by ovariotomy.

The *classes* of ovarian tumors, therefore, to which ovariotomy is adapted as a curative method, are—

1. All solid tumors.
2. All polycystic tumors.
3. All monocystic tumors also, which, for reasons above stated, should not be treated by any other of the before-mentioned methods.

The question whether ovariotomy should be resorted to in any given *case* of ovarian tumor, will be decided from the data given under the next topic—viz.:

B. *What are the Special Circumstances which Tend to a favorable Result of Ovariotomy, or the contrary? and what Conditions forbid it?*

B. The circumstances tending to a favorable result, or the contrary, after ovariotomy, will be better appreciated if we first examine—

The Causes of Death in Case of those who Die after the Operation.

Of the one hundred and fifty cases of ovariotomy of which I have collected the statistics, fifty-one died after the operation; and from the following causes:

Peritonitis . . .	12—23$\frac{27}{51}$ per ct.		Strangulation of intestine in wound	1
Septicæmia . . .	9—17$\frac{33}{51}$ "		Diarrhœa	1
Shock or collapse .	7—13$\frac{37}{51}$ "		Erysipelas	1
Exhaustion . . .	7—13$\frac{37}{50}$ "		Tetanus	1
Shock & Septicæmia	1—09 "		Ulceration through bladder . .	1
Hæmorrhage . .	1—09 "		Unknown	9

Thus peritonitis destroys nearly one-fourth of all who die after ovariotomy (and some statisticians say even forty-three per cent.); septicæmia (blood-poisoning) destroys about one-sixth of all; shock, collapse, and exhaustion, each over one-eighth. I use the term septicæmia as being here more accurate than pyæmia.

Hæmorrhage from the pedicle is said by some former statisticians to cause one-sixth (and even one-fourth) of all the deaths after ovariotomy. It is, however, an interesting fact that but one of the fifty-one deaths in my collection of cases is attributed to that cause. This would indicate that better precautions are now taken against hæmorrhage than formerly, in the management of the pedicle. It is, however, not improbable that some of the nine cases of death from causes not known were due to hæmorrhage; and some of the instances of septi-

cæmia may have been the secondary result of a hæmorrhage not sufficient rapidly to prove fatal as such. In some of the cases of death from exhaustion, the actual cause may have been a slow bleeding internally. The other causes of death do not admit of classification. I may add that bilious vomiting has also proved fatal, especially in cases in which the clamp has been so applied as to drag upon the uterus.

It follows, then—if we ascribe only five per cent. of the deaths after ovariotomy to hæmorrhage—that peritonitis, septicæmia, shock and collapse, exhaustion, and hæmorrhage, together, cause seventy-three and one-fourth per cent. of all such deaths. And all the special circumstances of the individual patient which tend to prevent the occurrence of these causes, just so far tend to render the operation successful. I shall consider the circumstances affecting its result under the following heads:

Personal conditions.
1. Duration of the disease.
2. Present general health of the patient.
3. The kind of tumor, its size, and the thickness of the abdominal walls; and effects of previous tappings.
4. Other pathological conditions complicated with the ovarian disease.
5. Age of the patient.
6. Temperament and mental state.
7. Married or single.
8. Previous diseases.
9. Certain external circumstances.

1. The fact that the disease has had a *duration of two to four years*, is to be regarded as of favorable import, if the operation be performed. If, however, the tumor has grown very slowly for many years, and the patient is advanced in life, it may not be deemed necessary to operate at all. On the other hand, if the tumor has not been detected more than a few months, and is still quite small and slowly enlarging, the operation is not yet to be performed.* Rapid growth is to be accepted as of unfavorable import at any age, though it is most common in young women (under twenty to twenty-two); since it is usually associated with a delicate constitution, or at any rate diminished

* In one of I. B. Brown's cases, the tumor had been detected eight weeks before, and had given some trouble for six weeks only. Died on ninth day, of peritonitis. London Lancet, April, 1863, p. 256.

power of recovery from the operation. Dr. McRuer of Maine* thinks that ovarian tumors of a medium duration and rate of development are the most favorable for the operation. I should say the longer the duration of the disease, *cæteris paribus*, the better; unless it has so reduced the general health as to endanger death from shock, collapse, or exhaustion.

2. In regard to the state of the patient's *general health* most favorable for the operation, directly opposite opinions are held by ovariotomists. T. Spencer Wells, Dr. Black, and I. Baker Brown, hold that the more robust and healthy the patient is, the better; and Mr. Hutchinson says the earlier you operate the better—a view opposed to the one I have just advanced. On the other hand, Dr. Tyler Smith, Dr. W. L. Atlee, and Mr. Erichsen, think the results are more favorable if we wait till the general health is somewhat impaired by the ovarian disease—in which opinion I fully coincide.

As this is a question of the highest practical importance, I shall examine the grounds on which, I think, it must be decided:

1st. If we delay till the health is somewhat impaired, and the patient is slightly anæmic (at least not having much pressure of blood in the vessels), we diminish the risk of peritonitis, which, we have seen, destroys one-fourth of all who die after the operation. Hæmorrhage is also less liable to occur. Dr. Black indeed remarks, in connexion with this subject, that there is "less predisposition to inflammation with strength than with weakness."† Unless he means extreme weakness—a condition no one would recommend—I think his statement entirely unfounded; and even with that qualification, it should be received with some degree of skepticism, since death after the operation in cases of great debility, generally results not from peritonitis but from shock or exhaustion, which together also destroy one-fourth of all who die. Dr. Black made the preceding remark in connexion with a successful operation of his on a patient who had had the tumor ten years without affecting the general health. Surely we cannot object to the result; but

* American Journal of Med. Sciences, July, 1859, p. 287.

† London Lancet, Oct. 1863, p. 650.

in another precisely similar case, I should delay for the preceding, and more especially for the following reasons:

2d. If the ovarian disease has not yet deranged the *general health*, the patient is certainly in no immediate danger from it. She may, and probably will live several months, and perhaps years, before she will be in danger. Is it right, therefore, for me to incur the risk of her dying in a few days in such circumstances? Certainly not, unless it can clearly be shown that the risk is constantly becoming greater from delay, and which I shall show is not necessarily the case. If, then, we are tolerably certain that the patient will live several months, or even some years, without the operation, let us make sure of that amount of life first, and operate when it becomes apparent that the operation is the only means of much further prolonging life.

3d. By deferring the operation so long as the patient remains comparatively comfortable, we gain time to perfect our diagnosis, if there be any special difficulty in the case. A single tapping will aid us very much in this respect; and frequently, also, the patient rallies after it, if quite weak before, and thus, also, we are enabled to postpone the operation of ovariotomy indefinitely. I should always prefer this course, if admissible, in every case.

4th. Those who operate in robust health do not have better success than those who wait for the general health to be somewhat impaired. Besides, some of the successes of T. S. Wells and I. B. Brown have been achieved by a free bleeding of their patients when inflammatory symptoms came on—doubtless a wise expedient if a patient *is* operated upon in full health, as it puts her on a par with one who had previously been somewhat anæmiated by the disease. I cannot, therefore, help regarding it as fortunate that most of every operator's patients do not come under his observation till the general health is impaired; so that, independently of his theory on the question under consideration, he is usually obliged to operate in this condition.

5th. Finally, certain comparative facts have an important bearing on this question. If we arrange all amputations under two classes—pathological amputations (those necessitated by diseases), and those of expediency (performed on account of tumors, elephantiasis, club-foot, &c.) we find that of amputations

of the lower extremity alone, forty per cent. of those of expediency prove fatal; while of the pathological amputations, only fifteen per cent. prove fatal.* If ovariotomy is performed while the patient is in full health, it becomes an operation of expediency; while if we delay till the health fails somewhat, it is analogous to the pathological amputation, and is therefore more successful. My statistics and my observation warrant me in asserting this principle as also applicable to ovariotomy.

My reasons, then, for delaying the operation till the general health is somewhat impaired, are—the danger of peritonitis and hæmorrhage is thus diminished; if the patient will probably live months, we should make sure to her a part of this period of life instead of at once risking her death within a few days; we may thus have time perhaps to tap the patient and to perfect our diagnosis, and shall, besides, have a better chance of success after all, as is shown by the relative success of pathological amputations and those of expediency. On the other hand, we are of course not to delay till the health is entirely broken down, lest death ensue from shock or exhaustion.

Except the slight debility of the patient, as just described, it is desirable that she should be as nearly healthy as possible at the time of the operation, and especially so far as the digestive function is concerned. And if this is deranged, time should first be taken to correct the derangement. If menstruation has also been arrested by the ovarian disease, I consider it a favorable condition. The uterus and ovaries being inactive, there is less danger of inflammation.

Let us now, on the other hand, consider the dangers and disadvantages, if any, which may result from delaying as I have recommended. We have to bear in mind that non-malignant ovarian tumors do harm only in two ways: (1) by exhausting the system, from the amount of the elements withdrawn from the blood; and (2) by mechanically interfering with the functions of the abdominal and the thoracic organs. I have cautioned against delaying so long as to risk extreme exhaustion; while I have also shown that a resort to simple tapping may often relieve for a time the other effects just mentioned, and

* London Lancet, May, 1859, p. 387.

thus enable us to defer ovariotomy for the present.* The strongest argument against delay is the assumption that, of course, the adhesions will thus become more extensive and increase the danger of the operation.

Certainly we must eliminate from this category all cases in which ascites is complicated with the ovarian tumor, since in such, few if any adhesions are formed at all, and can hardly increase after the fluid is abundant. Nor is it by any means certain that a tumor not thus complicated, which already distends the abdomen, and is probably already somewhat adherent, will be essentially more so a few months, or a year or two hence; but I return again to this point. The only case in which we can rationally assume that a delay will secure a decided increase of adhesions, is a case uncomplicated with ascites, of an ovarian tumor still small, and movable in the abdominal cavity. But shall we accept Mr. Hutchinson's rule, and operate as early as possible, lest there may be adhesions a year or two hence? I have given the reason for first securing this addition of a year or two to the patient's life, and then dealing with the case as we find it.

But suppose adhesions actually *do* become more extensive and more firm in case of a tumor already adherent, or of one now too small to be so, what practical importance really ought to be attached to their existence? Very little in most cases, and for the following reasons:

1st. These adhesions are almost always merely *physiological*, and not pathological; *i.e.* they are formed from an exudation poured out between the peritoneal surfaces brought into contact, and kept at rest in contact, after the tumor has attained to a large size, and for the purpose of supporting the tumor in that position; they are *not the result of inflammation.* The sac and the peritoneum over it sometimes become inflamed, but not so frequently as to justify the expectation that inflammatory adhesions exist in any given case, unless symptoms of inflammation have clearly pre-existed. Hence we expect to find large tumors adherent at the upper part (and probably also on the sides) as a matter of course, and equally if there have been no signs of inflammation. But unlike inflammatory

* See my paper before referred to, in Bulletin of Acad. of Med., March, 1864.

adhesions, the union is not very firm, and unless of very long standing, they are but very slightly vascular. Hence, they are also generally easily overcome, and do not lead to any troublesome hæmorrhage.

2d. But if they are very extensive and very firm (even if inflammatory), so as to require much force to overcome them, and to lead to hæmorrhage, demanding the application of ligatures—all this does not essentially modify the result of the operation. Even the danger of peritonitis is not perceptibly increased, as we might *à priori* expect would be the case. On the contrary, it appears rather to be diminished; for the peritoneum, where the surfaces were in contact, has lost its epithelium, and is no longer a serous membrane.

3d. Experience proves that recoveries are very frequent in cases of extensive adhesions. They existed in all of my own cases; in the last, more extensively than in any case I have seen reported; but no bad symptoms occurred which were referable to them. And I think it should be laid down as a *rule* that we should not be deterred from finishing the operation by the existence of adhesions.

There may, however, be two exceptions to this rule. (1.) If the adhesions are so extensive as very much to prolong the operation in case of a much debilitated patient, the risk of death from shock or collapse may induce us to desist. (2.) If the tumor is adherent to the liver, the bladder, the uterus, or the large intestine, and cannot be detached without injury to these organs respectively, we should desist. In most of such cases, however, the adherent portion of the sac may be cut out and left attached to the organ to which it adheres, without any subsequent inconvenience. I therefore fully concur with Dr. W. L. Atlee, who writes me: "I am never deterred from operating by adhesions. Unless they are visceral, I think they should not be regarded. Indeed, I think peritonitis is less likely to occur in these cases than when the peritoneum is wholly intact. Its character is entirely changed, and it is no longer a serous membrane." And in reference to the point last alluded to, he says: "I have left portions of the sac as large as the hand adhering to the viscera, when too firm to be removed—with good results."

Up to the present time, therefore, I should inculcate the rule—*Wait until the general health of the patient is somewhat impaired.*

3d. The *kind* of tumor, its *size*, and the condition of the walls of the abdomen, are important matters in this connexion. Monocystic tumors are far more favorable than the polycystic. If the tumor is pretty *large*,* the parietes have of course been distended; which is a decided advantage in three respects. (1.) The peritoneum having been stretched for a long time, has become less sensitive, as explained by Dr. Nelson of this city; and thus peritonitis is less likely to ensue. (2.) The walls collapsing after the operation, less mischief will result if tympanitis, coughing, or vomiting should follow.† (3.) The walls of the abdomen will of course be *thinner*. For, a considerable thickness of the abdominal walls (even over one inch) may render it impossible accurately to coapt the inner edges of the incision, and thus induce a fatal result. Of course the difference in thickness depends, aside from the thinning by the distension, upon the amount of adipose tissue under the skin; and which is seldom sufficient to give trouble, except below the umbilicus. Here, then, we find still another reason for delaying the operation till the flesh as well as the general health is somewhat reduced. My last patient was greatly endangered by this cause; the walls being, for about three inches, two inches thick.‡ As the incision was not perfectly closed internally, suppuration, inducing septicæmia, occurred; from which the patient, however, slowly recovered under a treatment hereinafter specified.

The complication, therefore, of *ascites* with ovarian tumors is favorable, inasmuch as it both distends and makes thinner the abdominal walls. It has already been stated that it also, to a great extent, prevents the formation of adhesions, while the tapping necessitated by it enables us to perfect our diagnosis, and often to ascertain the existence of adhesions. Does tapping itself for ascites increase the risks of ovariotomy after-

* *I. e.*, at least as large as the gravid uterus at eight or nine months.

† Either in preventing the incision from healing, or in producing a hernial protrusion through it, as in a case in which I extirpated the body of the uterus.

‡ Amer. Jour. of Med. Sciences, July, 1864, p. 50.

wards? I think not. But possibly the peritoneum, if ascites had coëxisted with the ovarian tumor, may secrete some amount of the same ascitic fluid after ovariotomy; this fluid may undergo decomposition instead of being reabsorbed, and septicæmia may result. I think this a very rare occurrence; but I have seen two cases of septicæmia thus induced, both of which recovered.

In this connexion, also, the question is suggested whether previous tappings of the ovarian sac or sacs renders the operation of ovariotomy more dangerous. I. B. Brown states that those patients do badly who have been tapped very many times. But I suppose it is the exhaustion produced by the secretion from the blood of such large quantities of fluid as patients, very many times tapped, must have lost, that produced the bad result; and not the tapping. The latter is itself, however, by no means to be regarded as unattended with danger, especially when performed on a patient for the first time; since, of first tappings, at least one out of seven proves fatal.* It is remarked that adhesions are produced by tapping. But when this is the case, they are of slight extent around the punctured point; and when once formed, if the subsequent tappings are made at the same point, as they should be if practicable, the risk of the operation is thus quite effectually guarded against. On the other hand, the operation of tapping may, by collapsing the sac, prevent the formation of adhesions for the time, or even overcome to some extent those already formed. Finally, according to my own statistics, previous tappings do not materially increase the mortality after ovariotomy. Of fifty-seven who had been tapped from one to twelve times, twenty died after ovariotomy, or 35 per cent.; of twenty-two of the preceding, who had been tapped three to twelve times, eight or 36.3 per cent. died; and of thirty-five who had been tapped once or twice only, twelve or 34⅓ per cent. died. A single patient who had been tapped twelve times, recovered. We notice, however, a higher mortality in those who had been tapped three times or more, than in those who had undergone the operation but once or twice. Single tappings gave a mortality of only 14.2 per cent.

* See my previous paper, already alluded to.

4th. If any grave *pathological condition is complicated* with the ovarian disease, as tuberculosis, organic heart disease, cancer, or any acute disease, ovariotomy is of course not to be attempted. But I wish especially to remark that albuminuria does not, as a matter of course, forbid it.

5th. The *age* of the patient has a very important bearing upon the operation of ovariotomy. Dr. Clay, of Manchester, states that the majority of those of his patients who died of shock and of peritonitis were young females; while those dying from prostration were chiefly elderly women. He, however, and I. B. Brown, think that age does not much influence the result; his success having been about equal from the age of 16 to 57 years.* In regard to extremes of age, I may remark that Dr. Atlee has operated on a patient of only 15 years; T. S. Wells, on one of 61 years; Mr. Hutchinson, of 65 years; Dr. Atlee, of 69 years; and Dr. Bennett, of 75 years—and they all recovered. My own statistics afford the following results in 99 recoveries:—

Under 20 years,	8	cases—	4	recovered—	50	per cent.
20 to 25 "	16	"	12	"	75	"
25 " 30 "	13	"	10	"	76.9	"
30 " 35 "	24	"	11	"	45.8	"
35 " 40 "	20	"	16	"	80	"
40 " 45 "	10	"	6	"	60	"
45 " 50 "	7	"	4	"	57.1	"
50 " 55 "	10	"	8	"	80	"
55 and upwards,	8	"	7	"	85	"
Age not stated,	34	"	21	"	61.7	"

Thus it appears that the most unfavorable epochs for ovariotomy are, under 20 years; from 30 to 35; and from 45 to 50—only ½ of all operated upon in these periods recovering. The most favorable epochs are, 35 to 40 years; and 50 years and more—the recoveries amounting to even 81$\frac{11}{19}$ per cent.; while 25 to 30 years, 20 to 25, and 40 to 50, are next in order in this respect. Perhaps an explanation of these results may be found in the following suggestions:

Women less than 20 years old have not the vigor to endure the shock of the operation, as they have during the following ten years. Those of 30 to 35 are, many of them, exhausted more

* London Lancet, June, 1863, p. 408.

or less by child-bearing; if not thus exhausted, woman attains her maximum of physical strength during the age of 35 to 40. Then ensue ten years of doubtful health, connected in part with the cessation of the catamenia. And these being past, and the uterus and ovaries henceforth inactive after 50 years, the operation is again as well borne (or better, perhaps) as at any earlier period. We have also seen that the arrest of menstruation by the progress of the ovarian disease, is a favorable condition.

6th. Another all-important element of success in ovariotomy is a sanguine *temperament*, and hopeful state of mind on the part of the patient. If, after comprehending all the risks of the operation, she confidently expects to recover from it, her mental state alone will enable her to triumph over much of its danger, and may secure its success. She should also feel that the operator selected is *the one* of all to save her. So important do I consider these matters, that I should decline to operate on a patient who despaired of recovering. A quiet, courageous, and amiable disposition is also a very important element of success; while a restless, irritable, timid, or very excitable woman is far less likely to recover. Any affliction or permanent cause of mental anxiety must also be taken into account. Far too little attention has, I think, been paid to these considerations in deciding whether or not to perform the operation in special cases.

7th. The condition of the patient as to being *married or single*, also affects the result of ovariotomy; and, as we would predict, the unmarried have the advantage. Of 116 of my collected cases, 64 were married, and 52 unmarried; the results being as follows:—

Of 52 single,	38 recovered—	73¼	per cent.
" 64 married,	28 "	59⅜	"

Here there is a difference of nearly 14 per cent. in favor of the unmarried. The majority of patients between 20 and 25 years mentioned on p. 28 (12 out of 16), were single; while the majority above 45 were married; except 7 patients between 45 and 50, of whom 5 were single. All the patients under 20 years were single; of whom, however, 50 per cent. died.

8th. The *diseases of the patient previously* to the invasion of ovarian disease should also be alluded to here; of course, however, as influencing the result of ovariotomy unfavorably. I have to mention here all acute inflammatory diseases, especially peritonitis, puerperal or otherwise; a tendency to inflammatory affections in general, or to high febrile reaction from slight causes; habitual torpidity of the kidneys; irritability of the stomach indicated by habitual nausea or vomiting; a tendency to diarrhœa; an habitually inactive state of the skin; and all that we generally include under a cachectic habit. All these should, according to their degree, dissuade from the operation. The fact that ovariotomy, or any other severe operation, has recently been performed,* should induce delay, at least. I. B. Brown also states, that if the fluid taken from an ovarian sac by tapping is found to be all albumen, the operation generally terminates fatally.†

9th. Finally, the *external circumstances* of the patient, including her social position, are also important as affecting her state of mind, and the comforts required after the operation. But the mere external surroundings of the patient being mainly under our control, will be specified under the head of Preparatory Treatment; this, as well as the after-treatment, having quite as much influence on the result of the operation as the circumstances I have specified.

B. *The Conditions forbidding the Operation of Ovariotomy.*

Some of the conditions specified under the head of previous diseases, if extreme, come also under this. We may add, great emaciation; prostration, with a small rapid pulse, red tongue, and diarrhœa; tuberculosis or cancerous deposit in any part; organic disease of heart, liver, brain, or kidneys; unwillingness to have the operation performed; and despair of recovery. I. B. Brown mentions any general skin disease, and a recent previous operation of ovariotomy, as forbidding the operation so long as these circumstances exist; and I add a recent at-

* T. S. Wells operated on a patient 8 months after she had been operated on by I. B. Brown. She died of septicæmia. Dr. Atlee operated successfully on one of Dr. Clay's patients sixteen years after the first operation.

† London Lancet, May, 1861, p. 110.

tack of peritonitis, or tenderness still remaining from such an attack.

Recapitulation.

1. *Conditions favorable for Ovariotomy.*—Slow progress and prolonged duration of disease (two to four years); health somewhat impaired by its progress; menses arrested by it; final cessation of menses; large size of tumor; walls of abdomen distended and thin; coëxistence of ascites; age of patient between 25 and 30, 35 and 40, or over 50 years; sanguine temperament; quiet, cheerful, and courageous disposition; confident expectation of recovery; desirable social position and external circumstances; not married; no previous diseases; no adhesions.

2. *Conditions not favorable, but not decidedly affecting the Result.*—Adhesions, unless visceral, or unless very extensive, in prostrated patients; ascites (in much debilitated patients); previous tappings of the tumor; albuminuria from renal congestion.

3. *Unfavorable conditions.*—Very robust health; rapid progress of disease; small size of tumor; thick abdominal walls (1½ to 2 inches); age of patient less than 20, between 30 and 35, or 45 and 50 years; married; previous diseases before specified; fluid of tumor being albumen entirely; melancholic temperament; excitable, irritable, timid, desponding disposition; doubtful of recovery; penury; visceral adhesions; decided emaciation, and debility.

4th. *Conditions forbidding the Operation.*—Excessive emaciation; great prostration, with the tongue and pulse as before described; colliquative diarrhœa; tubercular or cancerous deposit; organic disease in any important organ—especially Bright's disease; general skin disease; recent peritonitis; recent operation of ovariotomy; utter despair of recovery.

I believe, therefore, with Dr. Tyler Smith,* that we can now pretty accurately calculate the chances of recovery. But shall we always decline to operate in every case in which the conditions are unfavorable? By no means. If the conditions are not such as to forbid the operation, and the patient, after understanding all the risks in her case, insists on our giving her

* London Lancet, June, 1863, p. 392.

what little chance she may have of thus prolonging life, we are not at liberty to refuse to operate; it being done with a full understanding on the part of all interested, on what grounds it is undertaken. Thus we shall not have to regret our action even if we fail; while we will sometimes have the intense happiness of having saved one who was ready to perish.

III. HOW SHALL THE OPERATION OF OVARIOTOMY BE PERFORMED?

Though ovariotomy is the most formidable operation ever attempted, it has more frequently than any other important one been thoughtlessly undertaken and recklessly performed. It does not, however, require the highest degree of mere operative skill; but, from its many unforeseen complications, it demands experience, and the utmost cautiousness and care.* I next adduce the teachings of what I consider the most enlightened experience on:

A. *The preparatory management of the case.*

B. *The operation itself.*

A. Some operators undertake the operation without any regard to the circumstances I am about to specify, except, perhaps, that a laxative is given the evening before it; while others insist on some other points which to most would appear frivolous. My own impression is, that in performing the most formidable operation known to surgery, we are bound to take every possible precaution which commends itself on rational grounds, against an unfavorable result; and with this view I shall consider separately: (1) the preparatory treatment; and (2) the preparatory arrangements at the time of the operation; and shall sometimes quote my own experience, as the directest method of enforcing my own conclusions on disputed points.

1. *Preparatory Treatment.*—There is quite a diversity of opinion in regard to the amount and the kind of preparatory treatment required. I have already spoken of the importance of having the digestive organs in as healthy a condition as pos-

* "It does not require great surgical skill, but plenty of nerve."—*I. B. Brown.*

sible; and this secured, I have not thought it necessary to make any change in the habits of the patient till the second evening preceding the operation. At that time I have given a full dose of castor oil to evacuate the alimentary canal three or four times the day before the operation (expecting no further action of the bowels for six or seven days after it); and meantime giving the patient only milk porridge* as nourishment, and securing sleep by the administration of an opiate, if deemed necessary. I give preference to the porridge, because it does not form any gas in the intestines, and they are therefore found collapsed, and do not protrude to give trouble during the operation. Dr. W. L. Atlee, with an experience of more than one hundred operations, writes: "The only preparatory measures I adopt are, the administration of the perchloride of iron for ten to fourteen days before the operation; castor oil the day previous to it; and opium the night before, and again one hour before the operation."

I. B. Brown suggests a succession of warm baths for a week or ten days before the operation, to secure a more active state of the skin and to prevent internal congestion. Dr. Black gave acetate of ammonia for a week previously, with the same view; and twice during that week a powder of Hydrarg. cum creta and ox-gall.†

2. Under the *preparatory arrangements*, I have to consider the time, place, state of the atmosphere, preparation of the apartment, and of the patient herself.

1st. The most favorable *time* in the year excludes both the coldest and the hottest season. In regard to the menstrual cycle, the operation should not be performed under five or six days after it, nor less than eight or ten before it. The best time of *day* with this as other severe operations is, the afternoon, at such time that it may be finished by the daylight, and yet not many hours before the usual time of sleep for the patient.

2d. In regard to *place*, the country is more favorable than the city. If in the city, the operation should be performed at

* Equal parts of milk and water, boiled one hour, and thickened with flour.

† London Lancet, October, 1863, p. 548.

a private residence, or an Infirmary, with arrangements expressly for this operation; but never in a large hospital, unless in a room completely isolated from the rest of the establishment. The well known want of success of ovariotomy in the large London hospitals has not been due to a want of operative skill, but to the unfavorable influences of the hospital arrangements. This is now so well understood, that no surgeon operates unless the patient is isolated as just suggested; and the operation is deferred also, if there is any contagious disease or epidemic prevailing in the hospital at the time.

This last precaution must also be taken, even if the operation be performed in the country; especially if peritonitis, erysipelas, phlebitis, or dysentery is prevailing, or if the operator has made an autopsis of a patient who died of either of these diseases. Dr. T. Smith is of opinion that the peritonitis following ovariotomy is most frequently produced by malarious influences, and not by anything inherent in the operation itself; and that it is, therefore, preventible to a very great extent. He suggests that it is akin to puerperal fever, and perhaps subject to similar laws. The success of T. S. Wells and I. B. Brown is greatly due, doubtless, to their connexion respectively with two small hospitals with special arrangements for ovarian cases—the Samaritan Hospital, and the London Surgical Home.

3d. The state of the *atmosphere* (the weather) at the time should also be considered. The day should be bright and clear—at any rate not a stormy day, nor, in this latitude, with the wind from the north-east. I. B. Brown remarks that "the atmosphere must not, at the time, be charged with ozone."

The atmosphere of the apartment during the operation should be maintained at a temperature of seventy-eight to eighty degrees (Fahrenheit); and be kept moist, also, by the evaporation of water. Dr. Clay attributes much of his success to these precautions, and Dr. Atlee also adopts them; while T. S. Wells and Dr. Tanner think them unimportant. I. B. Brown thinks favorably of them if the operation is to be prolonged—a point we can very seldom settle beforehand. I shall continue to adopt them; for they certainly commend themselves on rational grounds as an element of success, though not

essential in every case. The peritoneum is naturally warm and moist; and it would be difficult for me to realize that it is just as safe to have it become cold (even chilled, perhaps) or dry, or both, when the peritoneal cavity is opened. Of course the high temperature and the moisture are not required after the incision is closed; but a pure air afterwards, and a temperature of sixty-eight to seventy degrees in the cold season.*

4th. The *apartment* should be large and airy, quiet, and well ventilated. Thorough ventilation is of the utmost importance. T. S. Wells attributes his remarkable success of late to the fact that he keeps a window of the room constantly open after the operation, irrespective of the season of the year. An *experienced nurse* is required after this operation, more perhaps than in any other case, to attend to these and to other particulars hereafter to be mentioned. All admit this in respect to a lying-in woman; but in these cases the importance of experience is often entirely overlooked.

5th. Among the arrangements for the operation I also include the artificial serum,† first used by me in 1855. It is intended to imitate the natural secretion of the peritoneum; and is kept at a blood heat, and used to thoroughly moisten the hands before they are introduced into the peritoneal cavity.

6th. Chloroform is almost invariably used in this operation in England; and ether most frequently in this country. Dr. Clay states that the former, after his operations, produces serious sickness, and doubts if it has contributed to his success on the whole. Dr. Black also reports a case in which it softened and disorganized the bronchial mucous membrane, and produced death in three days and seventeen hours.‡ Mr. Holt reports a case in which it produced fatal collapse, from which the patient did not rally at all.§ I have never seen Sulph. ether produce any sickness after this operation, or any other unpleasant effect,

* I. B. Brown has flannels applied to the abdomen during the operation to keep the air from entering the peritoneal cavity.

† Composed of chloride of sodium, ℥iv.; albumen (white of eggs), ℥vi.; water, ℥iv.

‡ London Lancet, April, 1857, p. 312.

§ London Lancet, January, 1860, p. 47.

though it sometimes produces retching when first administered; and shall still prefer it.

7th. Dr. Simpson describes a table appropriate to this operation. We only need a strong one of proper height, four feet long and about twenty inches wide, and covered with a folded counterpane, and provided with pillows. Of the instruments required, I do not speak particularly. I. B. Brown uses flannel blankets to absorb the fluid from the peritoneal cavity, as well as to cover the patient during the operation, if required. Dr. T. Smith also uses warm blankets to keep the intestines from protruding during the operation. For the former use I should prefer a soft sponge.

8th. Finally, the patient, having evacuated the bladder, and being dressed* as required after the operation is finished, is placed upon the table; anæsthesia is produced; and we proceed to

B. The Operation Itself.

I shall also discuss this subject somewhat particularly, under the following heads:

1. Position of the patient.
2. The incision.
3. Overcoming adhesions, and removing the tumor.
4. Management of the pedicle.
5. Closure of the incision, and dressings required.

1. *Position of Patient on the Table.*—Dr. T. Smith at first placed his patients in a sitting or semi-recumbent position. T. S. Wells objects that thus there is a greater tendency to syncope from the chloroform, while the intestines are also more liable to escape.†

I should keep the patient upon the back till the moment arrives for lifting the tumor from the abdominal cavity, when she is turned on the side on which the tumor is attached, as first advised by Mr. Hutchinson,‡ and it falls out mainly by its

* She should have flannel drawers on during the operation, and a flannel vest.—*I. B. Brown, on Ovarian Dropsy*, p. 159.

† London Lancet, May, 1861, p. 437.

‡ Mr. Hutchinson, however, turns to the side *opposite* the attachment.

own weight. Besides, the fluid does not, in this position, fall into the peritoneal cavity; and, therefore, we may thus sometimes freely incise a polycystic tumor, in order to diminish its size, instead of stopping to tap it.

2. *Of the Incision.*—This is made in the linea alba, below the umbilicus; but whether the long (6 to 12 or more inches) or the short incision (5 down to even 2 inches) is preferable, is still undecided. Dr. Clay of Manchester, and Mr. Walne, always use the long incision; T. S. Wells, Dr. T. Smith, Mr. Lane, and Dr. F. Bird, have preferred the short one. Mr. Wells objects to the long incision, on the ground of its greater exposure of the intestines, and its being followed by many more serious symptoms and a more protracted recovery. He tends to the other extreme, and uses a *very short* incision—in one case only 1½ inches long.

Now since Dr. Clay's success compares favorably with that of those who adopt the short incision, I infer that its precise length is not a very essential point in the operation; though an avoidance of either extreme would doubtless be safest. Dr. Clay, however, explains that he does not make an incision incommensurate with the size of the tumor to pass through it. Still, if it be somewhat longer than actually required for that purpose, and it is well united, as usual, at the end of forty-eight hours, I cannot perceive how its mere length can have any agency in producing "many more serious symptoms, and a prolonged recovery;" though I have made incisions 12 inches and 14 inches long.

I therefore accord with Dr. Atlee and I. B. Brown, who commence with an opening in the peritoneal cavity of about 2 inches, and then increase it as the case may require. If the tumor is not adherent, and is small—or if not adherent and capable of being reduced to a small mass by tapping—a small incision (2 to 4 inches) may answer the purpose. But if there are extensive and firm adhesions (especially if visceral), the incision should be large enough to enable us to *see* them; and if the tumor is large and cannot be much diminished by tapping, the incision must be enlarged to allow its passage through it. I think the only rule is to make the incision *long enough to secure the objects I have enumerated, and no longer.* In six of

Mr. Wells's consecutive cases, no incision exceeded 5 inches. I think, however, that if we insist on this limit in some cases of very large polycystic tumors, the delay, with the hæmorrhage from breaking down the mass, will endanger the patient far more than a length of incision sufficient to admit of a prompt completion of the operation.

I should not, I think, here omit two remarks in regard to making the incision, viz. that the peritoneum should not be divided till *all* hæmorrhage from the parietal vessels has ceased; and that when divided, it should be done by *scissors* (on a grooved director) instead of a knife. I. B. Brown, in one of his cases, found a coil of intestine between the tumor and the parietal peritoneum, and says he certainly would have injured it had he used a knife.* The reason for the preceding rules is, that—

The incision should be regarded as merely explorative until it enables us to determine whether we are to complete the operation or not. For it is impossible to be *perfectly certain* in regard to its completion till we see the tumor and ascertain the amount and kind of adhesions. If we decide, on examination, *not* to remove the tumor, it is very desirable not to have let any blood fall into the peritoneal cavity; since it may be impossible entirely to remove it, and thus left, it may produce a fatal septicæmia. The bleeding vessels (mere veins) may be ligated to save time; and the ligatures subsequently removed when the incision is being closed.

Another rule also follows from the preceding, in regard to diminishing the tumor by tapping during the operation. If this is done before the question of removal is decided, always effect it by a small trocar; so that in case it is decided afterwards to leave the tumor, the puncture can easily be closed by a silk suture, cut short. If a very large instrument is used, and the tumor left, the fluid may still continue to flow into the peritoneal cavity (the trocar sometimes, also, producing copious hæmorrhage), after the incision is closed—with a fatal result. But when it is fully decided to remove the tumor, then the larger the trocar the better; even if one-half to three-fourths of an inch in diameter.

* London Lancet, May, 1863, p. 282.

If the tumor cannot be removed, the incision is closed, as directed further on. And here the question arises as to the danger of explorative incisions. Of course the danger is very much diminished by adhering to the rule I have given; since we proceed more cautiously if feeling that we may decide at any point to stop. They are quite dangerous only in the practice of inexperienced operators, as the following facts will show: Dr. R. Lee reported sixty cases in which gastrotomy only was performed; of which nineteen died.* Mr. John Clay includes one hundred and five cases in his statistics, in which the tumor was not extirpated; of which twenty-seven died.† Of thirteen cases in which the removal of the tumor had been abandoned in Ohio, up to Nov., 1859, seven proved fatal.‡ On the other hand, Dr. A. T. Barnes, of London, had had five cases in which the tumor could not be removed; and all the patients recovered from the operations. T. S. Wells had two similar cases, and both recovered.§ Dr. F. Bird made exploratory incisions in eighteen cases; all of whom recovered without injury.‖ I. B. Brown thinks the question of exploratory incisions has been unfairly treated by the profession. He had never seen a fatal result follow them, and thinks they should be commended and encouraged and not condemned.¶

3 *Detaching the adhesions and removing the tumor.*—I assume that unless the adhesions are very slight, or the tumor small and the incision comparatively large, there is no way to determine the extent and location of the adhesions without *passing the hand into the peritoneal cavity, and around the tumor in all directions.* And it is better to do this before tapping the tumor to diminish it; unless found impracticable from the distension of the abdominal walls by its large size, in which case use the *small* trocar at the least vascular portion visible. Before this is done, however, ascertain positively whether the tumor is ovarian or not, since uterine tumors sometimes cannot

* London Lancet, October, 1858, p. 286.
† London Lancet, April, 1861, p. 323.
‡ Amer. Jour. of Med. Sciences, April, 1860, p. 575.
§ Amer. Jour. of Med. Sciences, April, 1862, p. 551.
‖ T. S. Wells, History of Ovariotomy; Med. Chirurg. Transactions, vol. XLVI.
¶ London Lancet, June, 1863, p. 468.

be distinguished from the ovarian by the sight, or even by the sight and touch together. I think the best way, if any possibility of doubt exists, is to search for the *uterus* at once, and ascertain whether the tumor has any direct continuity with that organ or not.

Nor is there any objection whatever, so far as I am aware, to introducing the hand as just suggested; it being previously moistened with the artificial serum I have described, or with simple warm water. If, however, the portion presenting at the incision is a single, large, unadherent sac, it may be tapped with the small trocar and drawn out after it is collapsed, without introducing the hand at all.

Having decided to remove the tumor, though there are adhesions, the latter are next to be overcome; and if not very firm, this is best done by passing the hand freely around the tumor and breaking them up. In doing this, care should be taken to tear them off directly from the sac rather than from the surface to which they attached it; and this, also, without tearing the sac itself, since a copious hæmorrhage may thus result. Especial care must be taken when we attempt to detach adhesions from any viscus; and if firm and extensive, the incision should be elongated till we can bring the parts concerned into view. I have seen a tumor detached from the under surface of the liver, carry away a portion of the capsule of the liver with it, and thus expose a bleeding surface from which a fatal hæmorrhage took place. I do not know, indeed, how a hæmorrhage from such a surface could be artificially arrested. Almost equal precautions are necessary if the tumor adheres to the bladder, uterus, or any portion of the alimentary canal. If not to be detached from either viscus without great risk to the organ in question, the adherent portion of the sac should be cut out and left still adhering, as already frequently done by Dr. Atlee. Frequently bands are found too firm to be overcome by any amount of strength the operator can apply; and these are of course to be divided by the knife. The adhesions to the omentum are usually overcome without much difficulty; though at the risk of tearing that part.

Generally, adhesions *torn* away do not bleed; or if they do

at first, the hæmorrhage ceases on exposure to the air.* It is therefore a good rule not to ligate any bleeding vessel at once, since in a few minutes this may be found unnecessary. If, however, the oozing continues, the vessels may be ligated with fine silk, cut off short, and left in the peritoneal cavity. In my last case I thus applied twelve to fifteen ligatures, and no inconvenience followed. Not very seldom will we find the torn vessels of the omentum requiring this procedure. I. B. Brown uses silver wire in the same way.

If we expect to detach extensive adhesions without letting any blood fall into the peritoneal cavity, we shall almost certainly be disappointed. This is a matter of special import if the tumor is not to be removed, but of no importance if it is; since after the removal the blood is easily sponged out of the cavity.

The adhesions being overcome, the patient is turned on the side from which the tumor was developed, and the latter is lifted out of the peritoneal cavity and removed. Previously, however, to severing its attachment, we must have decided

4. *How to manage the Pedicle.*—The pedicle has been very differently treated by different operators; and even by the same at different times.

Dr. Clay, of Manchester, passes a double ligature of Indian hemp through the middle of the pedicle, ties one-half of it round each half of the latter, and brings the ends of the ligatures out through the lower end of the incision.

Dr. J. L. Atlee, of Lancaster, Pa., first (I think) applied the *ecraseur* to the pedicle in March, 1858. The patient, 61 years of age, made a rapid recovery.† His brother, W. L. Atlee, already often quoted, has since done this sixteen times.

Mr. Duffin, of London, in 1850, first applied the ligatures as Dr. Clay does; and then brought them and the pedicle out through the incision at the nearest point, and fixed it there by hare-lip pins.

Six or seven years ago, Mr. Hutchinson, of London, applied

* But if *cut*, they bleed. I. B. Brown had a patient die in three days after the operation, from bleeding, from an adhesion to the liver which he had cut off. London Lancet, April, 1859, p. 322.

† American Medical Monthly, August, 1858, p. 158.

a clamp to the pedicle, and fixed the latter in the incision; the clamp compressing the pedicle being placed externally across the incision.

Dr. Tanner, in one instance, tied the pedicle as above described, and left a portion of the tumor as large as the hand outside of the incision.*

I. B. Brown, in one case, applied silver wire to the pedicle and cut it close, and closed the incision. Patient died in 26 hours, of diarrhœa and vomiting. But he was, I think, the first to adopt the suggestion of Mr. Hutchinson; though he uses the common carpenter's callipers, as being lighter than the clamp. He removes the callipers generally on the third day (24 to 72 hours), and lets the pedicle fall back into the peritoneal cavity.†

T. Spencer Wells first adopted Dr. Clay's method; then Mr. Duffin's suggestion; then Mr. Hutchinson's clamp. After a few operations, however, he gave up the clamp, and returned to Mr. Duffin's method.‡ For the last six years he has used the clamp almost exclusively; removing it in 36 to 48 hours generally. In one case he was obliged to remove it in 4 hours, by the violent symptoms produced by its dragging upon the uterus. The patient did well.§

Dr. T. Smith first applied the double ligature like Dr. Clay. Next he cut it close, and closed up the incision, and the patient recovered. This had been done by Dr. D. S. Rogers, of New York, in 1829; by Dr. Billington, of New York, in 1835; by Siebold, of Darmstadt, in 1846; and recently by Mr. Fergusson of London. All these operations were successful; and very recently Dr. T. Smith has thus succeeded in 7 or 8 cases.‖

Dr. W. L. Atlee had, up to April 1, 1864, adopted Dr. Clay's method 41 times; had used the clamp 43 times; and the ecraseur 16 times, as already stated. He uses a ligature of three or four threads of saddler's silk, waxed but not twisted; and which I think preferable to the hemp ligature.

* London Lancet, February, 1861, p. 162.
† London Lancet, June, 1862, page 390.
‡ London Lancet, September, 1861, p. 182.
§ London Lancet, February, 1863, p, 139.
‖ London Lancet, May, 1864, p. 282.

It will be seen that the preceding methods may be reduced to five, viz.:

1. By the ecraseur.
2. By double ligature—Dr. Clay's method.
3. By double ligature, as modified by Mr. Duffin.
4. By double ligature cut short, as by Dr. T. Smith.
5. By the clamp.

Appreciation.—In appreciating the relative merits of the preceding methods, we must bear in mind that the first object in our treatment of the pedicle is to secure the patient against the risk of hæmorrhage therefrom. And, as I should not trust that essential and indispensable point to the doubtful hæmostatic effects upon the vessels, of the ecraseur, I shall eliminate that instrument from the calculation. On the other hand, no one can deny that the ligature affords the greatest possible assurance against hæmorrhage, it being always finally resorted to also when the clamp fails. In this respect, therefore, the ligature is to be preferred to the clamp.

But there are also two other incidental objects which demand attention in treating the pedicle, viz. to avoid any irritation in the peritoneal cavity which may lead to peritonitis or septicæmia; and to secure the closure of the incision as soon as possible. It is only in regard to these objects that the clamp becomes comparable with the ligature.

The *principle*, however, of the application of the clamp is the same as that of the application of the ligature by Mr. Duffin (the pedicle and ligatures being brought out through the incision and kept *externally* by a hare-lip pin till the pedicle sloughs off). Mr. Duffin's method, therefore, may also be dropped in the comparison; though it differs from the clamp inasmuch as it is safer against hæmorrhage, and the ligatures are not so conveniently removed, should we wish to remove them in two or three days.

There remain, then, for our appreciation, so far as the last mentioned two objects are concerned, but three methods—Dr. Clay's, Dr. T. Smith's, and the clamp. I pretermit Dr. Smith's method for the present, and compare the clamp with the use of

the ligature, as by Dr. Clay. And to do this intelligently, I have to consider:

1. The advantages, if any, of the clamp over the ligature.
2. The advantages, if any, of the ligature over the clamp.
3. The disadvantages of the clamp as compared with the ligature.
4. The disadvantages of the ligature as compared with the clamp.

1. The asserted advantages of the clamp over the ligature in regard to the two points above specified, are these: (1) It brings the constricted stump of the pedicle *outside the peritoneal cavity*; and (2) being removed in two or three days, the incision generally closes in its *whole extent in a few days* afterwards. T. S. Wells claims that the clamp "prevents absorption of the putrid matter from the sloughing stump, and the peritonitis connected with the effusion of fibrine around the latter. He hardly remembered any successful case in which peritonitis occurred, when the pedicle had been kept out." *

There can be no question that the clamp conduces to a more rapid closure of the incision than the ligature, and is in this respect to be preferred. But although it also brings the stump outside for a time, it does not thus necessarily prevent absorption and septicæmia, nor peritonitis. Mr. Wells's successful cases had not been attacked with peritonitis—an assertion many of those who use the ligature might also probably make, since peritonitis very often renders a case *unsuccessful*. He, however, had two cases of death from peritonitis out of 18 unsuccessful cases. I cannot agree with him that the fibrine exuded around the pedicle has any agency in *producing* peritonitis; and though the pedicle sloughs off and produces putrid matter when kept outside by the clamp, the assumption that this is equally the case when the ligature is applied and kept within, needs confirmation; and will be considered further on. Of 32 patients lost by Dr. Clay, 10 died of peritonitis; a fact attributable perhaps to the ligature, and perhaps to some other cause.

* L. Lancet, June, 1868, p. 409.

2. The advantages of the ligature over the clamp in regard to the two points now under consideration are, that it is applicable to every case, whatever the length or form of the pedicle; that it is easy of application, and does not give way, nor in any case or under any circumstances produce traction of the uterus, and its concomitant severe and sometimes fatal symptoms. These advantages I think more than counterbalance the single advantage of the clamp—its allowing a more rapid closure of the incision; unless the clamp does also prevent absorption of putrid matter as above quoted, while the ligature favors it—as I expect to show is not the case.

3. *Disadvantages of the Clamp.*—It cannot be used if the pedicle is quite short or very wide. I. B. Brown, however, in one case applied *four* clamps. It sometimes drags upon the uterus, producing headache, severe pain referred to the hip, vomiting, and other grave symptoms—even fatal collapse.* And if this traction is not made at first, it may occur at any time if tympanitis, vomiting, or cough should supervene.† The pedicle may also permanently adhere in the incision to the abdominal walls, and thus cause intestinal obstruction, as in one of Mr. Wells's cases.‡ This condition would probably interfere also with subsequent gestation. In two of I. B. Brown's cases, vicarious menstruation from the pedicle adhering in the incision, regularly recurred. § Besides, the pedicle when kept outside sometimes requires even twenty days to slough off, before the incision can at that point begin to close; as in Dr. Miller's case.—(*American Journal Medical Sciences*, April, 1859.)

4. *Disadvantages of the Ligature.*—The objections to the ligature are based on two asserted disadvantages: (1) It produces irritation in the peritoneal cavity and an exudation, and thus produces peritonitis; and (2) it causes the pedicle to slough off in the peritoneal cavity, thence to be absorbed as putrid matter, and thus produces septicæmia. It also of course prevents a rapid closure of the entire incision at the same time.

* As in Dr. Lyon's case; died in sixty-eight hours.—Lancet, November, 1863, p. 726.

† See four cases fatal from traction.—Lancet, April, 1861, p. 323.

‡ Lancet, July, 1860, p. 59. § Lancet, May, 1864, p. 282.

Admitting the last mentioned disadvantage only, let us see if the two preceding objections can really be substantiated.

1. On *à priori* grounds, we might expect that a ligature left in contact with a healthy peritoneum would produce irritation, and very likely peritonitis also. But in these cases the peritoneum is quite changed in its susceptibilities, as has already been explained. There is also an exudation of plasma around the ligatures and the pedicle.* But both irritation and exudation may occur without any symptom of peritonitis; and as a general rule certainly the latter does not occur. I have myself applied only 18 ligatures in this way; but not in a single instance were there any signs of peritonitis. We have seen that Dr. Clay, out of 32 fatal cases, had 10 of fatal peritonitis; while Mr. Wells had but 2 from peritonitis out of 18 fatal cases. But the difference could not have been due to the ligature. Very nearly one-fourth of all who die after ovariotomy, die of peritonitis; and Dr. T. Smith, who always uses the ligature, has had better success than those who use the clamp exclusively. It appears, therefore, that the first of the two objections to the ligature is not sustained by the facts. *Cæteris paribus*, however, the ligature would doubtless be more likely to irritate and produce peritonitis if the patient be operated on in robust health, as advised by Mr. Wells.

2. The assertion that the ligature produces a slough of the pedicle, which is separated and absorbed as putrid matter, equally needs confirmation. Dr. A. T. Barnes of London, says that no such sloughing takes place;† while Mr. Wells had a patient die in 30 *hours* after the operation from the absorption, as he believes, of putrid matter from the sloughing stump. Dr. Miller of Louisville, Ky., says "there is no reason to suppose that there is any difference" in the suppuration and sloughing, whether the pedicle is kept outside or left in the peritoneal cavity. I. B. Brown seems to admit the sloughing internally; but as he returns the pedicle into the peritoneal cavity on the second or third day, after slipping off the clamp —when the stump must of course be just as *dead* as if the liga-

* Dr. Black's case; copious exudation; no sloughing noted.—Lancet, April, 1857, p. 311.

† Lancet, 1861, p. 483.

ture had been in its place the same length of time (and more so, if there be any difference, from its exposure to the air)—he does not appear to fear any serious result from its sloughing and absorption. Indeed, he says there is as much danger from the air entering the peritoneal cavity as from the sloughing pedicle.*

Now I am unable to find the facts which sustain the idea either that any such sloughing of the pedicle, or therefore any such absorption of putrid matter, actually takes place from the use of the ligature—unless in very rare and exceptional cases. On the other hand, the considerations soon to be specified point to the opposite conclusion. In opposition, however, to Dr. Miller's idea, I think there "*is* a reason to suppose" that the suppuration and sloughing of the stump would *not* be precisely the same, if inclosed in a cavity of a uniform temperature and excluding the air, as if it were kept exposed externally. But the following facts seem to me to decide the question:

1. I have examined the reports of many fatal cases with special reference to this point; but in only a single instance is it stated that the stump was in a sloughy condition. In this case, however, the pedicle was kept outside, and was found "sloughing" 76 hours after the operation.

2. In a case in which I removed most of the body of the uterus, the patient died six days after, of strangulation of intestine through the incision, produced by violent cough. Here the *post-mortem* showed the stump included in the double ligature not to be sloughy; but, on the contrary, to be alive and nearly healed over by the exudation (just sufficient to afford the required amount of material) which had been poured out. Nor was there any trace of inflammation in the vicinity of the stump.

3. An experimental fact, quoted by Dr. Routh to show the great danger of septicæmia from sloughing of the pedicle if the ligature is applied, proves that no such sloughing occurs. He states that dead meat, even if fresh, when introduced into the abdominal cavity of an animal, produces putrid fever †—from absorption, of course, of putrid matter—the fresh meat becoming decomposed. But no such fever occurs when the ligature is used, as a general rule; therefore no such decomposition

* Lancet, April, 1859, p. 322.

† Lancet, September, 1861, p. 183.

or sloughing of the pedicle as is assumed occurs. Were there sloughing, the fever (from septicæmia) should be the rule and not the exception. Three of my own patients had septicæmia; but all from a source unconnected with the pedicle; though the ligatures were, in one case in which septicæmia did not occur, retained 18 weeks.

I am therefore compelled to conclude that the second objection also to the ligature—that it produces a putrid slough to be absorbed, and thus also septicæmia—is not sustained.

What then actually becomes of the constricted portion of the pedicle? As the portion of the femoral artery below the ligature in case of a flap amputation of the thigh that heals by first intention, is surrounded by exuded plasma and kept alive in spite of the constriction, so that when the ligature even cuts it off entirely by exciting ulceration, it does not slough, but becomes blended with the surrounding tissues and the organized exudation—so here, I suppose, the constricted portion of the pedicle is inclosed in the exudation and kept alive. In some cases, however, the pedicle seems merely to become atrophied and the ligature slips over the end. The former explanation is the more probable one in cases in which the ligature is retained a very long time. But if neither of these explanations be accepted, there is no fact to prove that the stump does not at any rate usually retain its vitality.

Conclusion.—Since the objection to the ligature as tending to produce peritonitis and septicæmia cannot be maintained as a general proposition, and since, if it sometimes does so (which is not proved), the clamp on the other hand, also, sometimes demonstrably produces even fatal effects—the clamp has no advantage over the ligature on the whole, with the single exception that it usually secures an early closure of the entire incision. The great superiority of the ligature over the clamp in preventing hæmorrhage more than counterbalances this advantage; and, therefore, I *still decidedly prefer the ligature to the clamp.**

* It has been remarked that Mr. Wells has had better success during the last six years, since he used the clamp. So has Dr. Clay during the same time, though he never uses it. Mr. Wells himself attributes his success to his after treatment, as will be seen.

I have examined this question at length, both on account of its great practical importance, and because I consider the principle on which the clamp is applied to be wrong in itself. I now again refer to Dr. Tyler Smith's method of treating the pedicle, viz. *applying the double ligature, cutting it close, leaving the stump in its natural relations, and entirely closing up the incision*, as the best of all methods; and the one to which all the others will, in my opinion, ere long give place.*

Having secured the pedicle, the other ovary is to be examined† (and removed if found to be similarly diseased); and the next inquiry is—

Shall fluid, if found in the peritoneal cavity, be removed before the incision is closed?

Shall all the *blood or dropsical fluid be removed from the peritoneal cavity* before the incision is closed? Certainly, if we would, so far as possible, secure the patient against septicæmia. Of 50 of the cases I have collated, in which the peritoneal cavity was carefully cleansed of fluid, 35, or 70 per cent., recovered; while of 18 cases in which it was not removed, only 10, or 55½ per cent., recovered. Some operators, however, remove any blood they can see on the intestines, but leave the rest and any dropsical fluid which may remain. I. B. Brown thinks the fluid left does no harm.‡ Mr. Wells and Dr. T. Smith remove *all* the fluid with care.

I. B. Brown removes it with flannel blankets applied to and among the intestines.§ Mr. Wells uses a soft sponge; and Dr. T. Smith also suggests a large one, that it may not be left and forgotten. Mr. Brown thinks he has seen the sponge do harm, as it does in the eye.

I should remove *all* the fluid in the peritoneal cavity, of both

* T. S. Wells objects to cutting the ligature close, since we "leave the ligature coming out to establish a channel through which the dead putrid remains of the tissues, strangulated by the ligature, might escape." Lancet, Sept. 1861, p. 183.

† A patient of Mr. H. Smith died on the 22d day after the operation, when the other ovary was found diseased and large as the first. Lancet, Nov. 1863, p. 719.

‡ Lancet, May, 1863, p. 338.

§ Lancet, May, 1863, p. 552–4.

kinds, with a soft sponge moistened in the artificial serum before mentioned; and to make sure that none is left, should examine every part of the alimentary canal implicated, under a strong light reflected into the peritoneal cavity by a mirror, after previously feeling for coagula and removing them with the hand. The fluid will, of course, mostly gravitate into the pelvis; and can be most easily found and removed if the patient be for the moment turned upon the side.* I have never seen any unpleasant results from the sponge or the manipulations just recommended.

Having thoroughly accomplished this object, we next attend to the—

5. *Closure and Dressings of the Incision.*

The incision should be closed as promptly as possible after the tumor is removed; and the first question here occurring is, Shall the sutures include the peritoneum also, as well as the other layers of the abdominal walls?

Dr. Clay† and Mr. Fergusson‡ do not include the peritoneum. I. B. Brown did not formerly, but now does sometimes; thinking it not important, since patients do just as well if it is not included.§ Dr. T. Smith thinks it will answer if the edges of the peritoneum touch each other. Mr. Wells always includes the peritoneum.

I have included the peritoneum (one-sixth to one-quarter of an inch in width) since my first operation in 1850, and consider this one of the most important points in the operation. If its edges are accurately brought into contact, as advised by Dr. T. Smith, and kept so, it doubtless answers every purpose; and his own and Dr. Clay's success may be greatly due to the fact that they have maintained this contact by their care in closing the incision. But I think the chances of success in this respect are very much increased by including the peritoneum in the

* In one case I turned the patient on the face in order rapidly to remove a considerable amount of dropsical fluid.

† Lancet, September, 1863, p. 608.

‡ Lancet, June, 1863, p. 398.

§ Lancet, February, 1861, p. 173.

sutures; and I am not aware that any undesirable result ever ensues. If the contact of the edges of the peritoneum is not maintained, the consequences may be very grave. The whole surface between its separated edges suppurates, and the pus falling into the peritoneum may produce septicæmia. This happened in one of my own cases; the abdominal walls being too thick to admit of a perfect adjustment of the peritoneal edges. Mr. Wells has also proved by experiments on dogs that if the edges of the peritoneum separate, the intestines become adherent to the whole surface denuded of this membrane. On the other hand, he has seen a case in which the incision suppurated through its whole length without any danger to the patient, since the peritoneum had been included in the sutures, and had united by first intention, and thus prevented the pus from entering the peritoneal cavity.

Dr. Atlee closes the incision by sutures of iron wire,* as also does I. B. Brown. Mr. Wells prefers hare-lip pins, as fixing the wounded surfaces more securely.† I have used silver sutures and pins at the same time, there being more of the latter if the abdominal walls are thick. If no portion is more than a half inch thick, perhaps sutures alone will answer; but even then I use two or three pins, as more reliable against distension, should it occur, from tympanitis; or against movements from vomiting or coughing. Each suture has another, or a pin, at a distance of a half inch from it. But instead of the common hare-lip pin, I prefer common steel needles of the required length, to which a small mass of common sealing-wax is attached as a temporary head. If very long ones are required, I have the middle portion annealed, and the needle somewhat curved. After they are introduced, the points are broken off, and they are fastened in the usual way.

Various *dressings* have been applied to the abdomen after the incision is closed. Dr. Black applied somewhat elaborate fomentations of poppy capsules, chamomile, etc., frequently changed, in one case,‡ and thought them very efficacious in

* His reasons for preferring iron to silver wire are to be found in the Amer. Journal of Med. Science, Jan. 1860.

† Lancet, January, 1860, p. 391.

‡ Lancet, October, 1863, p. 649.

conducing to the patient's recovery. Dr. Atlee and I. B. Brown apply the many-tailed flannel bandage. I have merely used a warm water-dressing, covered with oil-silk, and kept in place by a flannel bandage applied just tightly enough to be comfortable to the patient.

The patient is next put into bed and covered warmly. Before speaking, however, of the after-treatment, I will record some

Complications which may arise during the Operation.

I. B. Brown once tore one of the Fallopian tubes, at its junction with the uterus, while detaching adhesions to the latter. Free hæmorrhage occurring, he brought the separated surfaces together by two silver sutures and cut them close. The bleeding ceased and the patient recovered without a bad symptom. In his next case, his incision (six inches long) penetrated directly into the tumor, it being everywhere adherent in front; and twenty-two pints of albuminous fluid, tinged with blood, escaped. The callipers were applied; the patient was very sick after the operation; low peritonitis came on in the evening, and she died in twenty-four hours. The fluid, under heat, became all albumen.* In one instance he put four, and in another six sutures into a wound in the uterus, produced by detaching adhesions, and cut them short. The patients did well.† He put four clamps on a single pedicle in one case, and removed them in forty-eight hours; it being formed of bands extending to the sacrum, the pelvic fascia, and the top and sides of the uterus. He also peeled off adhesions six inches long from the bowel, and tied the vessels with silver sutures. Patient vomited bile but recovered.—A large adhesion was secured by a silver suture and then divided; no hemorrhage; recovery.† Whole omentum adherent in one case; tied a large portion with a silver suture and cut that portion off. Recovery.—Adhesions in all directions to intestines, which were also glued together by previous attacks of peritonitis. Omentum wholly adherent. Tied latter in three places with

* Lancet, August, 1861, p. 110.

† Lancet, April, 1863, p. 256; and his work on Ovarian Dropsy, p. 260.

silver wire, and cut a large piece off. Eight days after, fæces passed in large quantity through the lower part of the wound, and a large piece of sloughy intestine came away. A fistula remained for some time, but spontaneously closed. Recovery.* He has twice removed both ovaries with success. I have done so three times. The removal of the second ovary seems not essentially to increase the danger of the operation. In another case he tied a large portion of bleeding omentum and left it in the incision. A portion sloughed off in forty-eight hours, and a bleeding vessel was then tied. Portions still remained in the wound at the end of four weeks, but were gradually disappearing.†

Dr. T. Smith, in his sixth case, found the tumor (a single sac) in the broad ligament, with a broad pedicle. After tying the latter and dividing it, it bled freely from a rupture in it below and behind the ligature. Another ligature nearer the uterus arrested the bleeding. But peritonitis came on during the night, and the patient died in twenty-four hours. She had granular kidneys.‡

Mr. Wells having extensively torn the omentum, cut off a portion which did not appear healthy, tied four vessels with fine silk ligatures, and cut the latter short.§ Mr. Bryant also did the same, and the patient recovered.‖ In another of Mr. Wells's cases the proper pedicle could not be safely separated from the cæcum above, or the bladder below. He therefore put the clamp around the neck of the tumor, a large polycystic one, and removed it on the third day. The patient did well.¶

Mr. Simon tied two omental adhesions with silver wires cut short, and left them in the abdominal cavity. Recovery.*[2]

In my last case the omentum was everywhere adherent, and was much torn in separating it from the large polycystic tumor. I tied thirteen or fourteen vessels with fine silk ligatures and cut them short, returning the omentum into the abdominal cavity. Patient recovered.†[2]

* Lancet, April, 1863, p. 257.
† Lancet, April, 1863, p. 251.
‡ Lancet, September, 1861, p. 183.
§ Lancet, June, 1863, p. 399.
‖ Lancet, August, 1863, p. 528.
¶ Lancet, August, 1863, p. 530.
*[2] Lancet, August, 1863, p. 531.
†[2] Amer. Journal Med. Sciences, July, 1864, p. 50.

IV. WHAT IS THE APPROPRIATE TREATMENT AFTER OVARIOTOMY?

This is a question of the greatest importance. For, while the dangers of an operation carelessly performed may often be averted by the best management afterwards, no amount of operative skill or experience is likely to succeed if the patient is subsequently neglected or injudiciously treated. Impressed with this idea, I have uniformly refused to operate unless the patients were so situated as to remain under my care after the operation as long as might be necessary. Dr. Clay thinks the third, sixth, and ninth days after the operation are critical days;* and agrees with I. B. Brown that success is determined more by the after-treatment than by the operation itself.†

The patient, having been covered warmly in bed, may take thirty to forty drops of McMunn's Elixir of Opium, or one to two grains of opium, as soon as she has begun to rally from the operation and can swallow it well; and the catheter should be used every six hours, or oftener if the patient's sensations demand it. If vomiting occurs, Dr. Clay advises patience till the blood gets rid of the chloroform; simple drink, and as little food as possible. I have not seen this symptom after ovariotomy under sulphuric ether.

The hygienic management consists in keeping the apartment quiet and well ventilated, and giving appropriate nourishment. T. S. Wells has more recently kept a window constantly open, even in the cold season, with a fire burning; and to this precaution (not to the clamp nor his skill as an operator) he attributes his remarkable success; having saved fourteen out of the last fifteen, at the time when he called attention to this practice. I think the best nourishment is milk porridge, or beef tea, or broth, during the first five to seven days; no solid food to be given, as Dr. Clay advises, till asked for. It is not desi-

* London Lancet, June, 1863, p. 408.

† Up to April, 1863, Mr. Brown had operated nineteen times in the Surgical Home, and the same number of times in St. Mary's Hospital, and in private practice. Of the former, thirteen recovered; and of the latter, only six. He attributes the difference to *skilled* nursing and the better organization of the Home.—*Lancet*, April, 1863, p. 256.

rable that the bowels be evacuated in less than six or seven days after the operation; and an enema of soap and water—unless a dose of castor oil is preferred—may accomplish that object for the first time.

Sometimes the preceding treatment is all that is required, the opiate being repeated perhaps two or three times every twenty-four hours, during the first two or three days; the patient going on to complete recovery without a bad symptom. Generally, however, more medical, and often also surgical treatment is required. But shall we uniformly give opiates in full doses, as some operators recommend? Dr. Clay gives four grains of opium to begin with, and two-grain doses afterwards. I. B. Brown has given up this practice, as a *rule*, since opium after chloroform produces sickness; but pain, he says, is a greater evil than opium.* Dr. T. Smith gives a moderate amount of opium; and stimulants on the third or fourth day, if not contra-indicated by the existence of inflammation. My own opinion is, that just enough of an opiate should be given to overcome pain and restlessness and secure sleep, *and no more.*† Under this rule we should in some cases give but one grain of opium once to three times in twenty-four hours, for two or three days; while others would demand, perhaps, four times as much for twice as long a time. Decided narcotism should always be avoided. T. S. Wells gives opium, and by the rectum; I. B. Brown by the mouth. I prefer the McMunn's Elixir of Opium, and give it by the mouth. But if any irritation of the stomach exists, I give it by rectum, as I do opium itself in all cases. In this condition the nourishment should also be given in the form of enema.

Peritonitis is less dangerous and more amenable to treatment when traumatic, than the idiopathic variety. Still it destroys about one-fourth, as has been shown, of all who die after ovariotomy. If, however, it occurs seven to ten days after the operation, it assumes the latter (or zymotic) form. Occurring in the first two or three days in a patient operated on in full

* Lancet, September, 1861, p. 183.

† The practice recently recommended, of keeping up the action of the anæsthetic after the operation by the hypodermic injection of a solution of acetate of morphia, should be discouraged.

health, blood should be abstracted by cupping or leeches, as deemed best;* and then the acetate of morphia (Magendie's Solution) may be given in quantity to overcome the pain and all malaise. If the other form of peritonitis presents, tonics and stimulants may afford the only chance of recovery; and if septicæmia also coëxist, the treatment soon to be specified may also be required.

One of Mr. Wells's patients had "an oppression of the heart and lungs, and a pulse of 130 to 140" the next day after the operation. She had lost no blood at the operation; and was relieved by the withdrawal of ℥ xvi. of blood, and recovered.† Another patient coughed suffocatingly till she lost ℥ iv. to ℥ vi. of blood from the pedicle, which the clamp did not properly control. A ligature was then applied; the patient recovered.‡

Query.—Would these symptoms probably have occurred, and have required the loss of blood, had the health of the patient been somewhat impaired before the operation was performed?

If *hæmorrhage* occurs from the pedicle, the latter being kept outside, a ligature is of course to be applied to the bleeding vessel. If the pedicle bleeds within the peritoneal cavity—a very rare thing if the *ligature* is applied—the incision must be re-opened, the vessel tied, all the blood removed from the cavity, and the wound re-closed. At any rate, this alone gives the patient any chance of life.

A slow oozing, however, of blood from the vessels of the omentum, or some surface to which the tumor had been attached, is not uncommon; and is to be prevented, if possible, or diminished if not so, by quiet of body and mind after the operation. The effused blood becoming decomposed, may lead to septicæmia, in which case the treatment next to be specified may be required.

Septicæmia (here improperly termed pyæmia) destroys over one-sixth of all who die after ovariotomy. It results from the absorption of a decomposed fluid from the peritoneal cavity,

* I. B. Brown has had good results in two or three cases from the abstraction of blood; while Dr. Clay does not deem it advisable.

† Lancet, February, 1863, p. 189. ‡ Lancet, June, 1863, p. 399.

and may be produced, (1) by oozing of blood, as just explained; (2) by fluid from the tumor, left in the peritoneal cavity; (3) by ascitic fluid in that cavity; and (4) by pus in the cavity, in case the edges of the peritoneum separate internally and leave a surface between them, behind the closed incision, to heal by suppuration and granulation. This last alone is literally pyæmia. Others admit septicæmia from the absorption of putrid matter from the slough produced by the application of a ligature to the pedicle, according to Dr. Clay's and Dr. T. Smith's methods. I think I have shown that such is very rarely if ever the fact.

Since time is required for the fluid to become decomposed, and the peritoneum does not absorb it rapidly,* the symptoms of septicæmia do not appear under four to seven days, and in one of my cases not till the eighteenth. I can hardly imagine death to occur in thirty hours after the operation from this cause, as supposed in one of Mr. Wells's cases already mentioned.

The symptoms of septicæmia are those of pyæmia, so called; loss of appetite; coated, dry, and then red or dark tongue; great debility not otherwise to be accounted for; rapid, small, weak pulse; dizziness, a tendency to sleep, and a typhoid expression of countenance. In some cases, also, an accumulation of fluid in the peritoneal cavity can be recognised.

If the quantity of fluid to be absorbed is large, its absorption will prove fatal; but if it can be seasonably removed, life may be preserved. Even if it can be diluted with water and partially removed, the patient's chances must be so far improved, and life will be at least prolonged.

Acting upon these convictions, I first injected a solution of chloride of sodium (ʒj. to Oj.) into the peritoneal cavity of a patient much prostrated by septicæmia, in February, 1855. I

* As shown in my second case of septicæmia, Amer. Journal of Med. Sciences, April, 1863. One of Mr. H. Smith's patients lived thirteen days with an opening into the bladder large enough to admit two fingers, through which the urine constantly flowed into the peritoneal cavity, and thence through the incision. The flow of urine through the latter began on the eighth day after the operation. The opening in the bladder was doubtless produced by detaching an adhesion. During the first five days blood flowed by the side of the pedicle. Lancet, Nov. 1863, p. 719.

began with one quart of the solution, and then drew out the same amount of fluid with the syringe; though I soon found I could inject that or a larger amount (even two quarts) through a flexible bougie, and then, changing the position so as to bring the free extremity to a lower level than the one in the peritoneal cavity, convert it at once into a siphon through which all the fluid would freely flow out. The immediate relief from the first injection was very striking; the dizziness and stupor at once disappearing, though to return again in eight to twelve hours. I repeated the operation twice daily, and then once daily for a week, when the returned fluid no longer presented any odor of decomposition. When the fluid was unusually fœtid, I used a solution of the Liquor sodæ chlorinatæ (ʒ ij. to Oj.). The patient recovered rapidly from the time when the fœtor of the fluid was overcome.

In September, 1862, I again resorted to the same practice in a second case of septicæmia after ovariotomy. Here, I at first used a solution of chloride of sodium and albumen in water, as more nearly resembling the natural secretion of the peritoneum. But soon finding that the albumen itself became decomposed by admixture with the fluid in the cavity, I afterwards used either pure water, or a solution of chloride of sodium, or of the Liquor sodæ chlorinatæ, according to circumstances. In this case I found it necessary to continue the injections twice daily, and then once daily for fifty-nine days in all, when the fœtor ceased and the patient thereafter convalesced. In these two cases septicæmia was caused by the absorption of ascitic fluid secreted after the operation. In the first case, the symptoms appeared on the sixth, and in the second, on the eighteenth day.

A third case of septicæmia, produced by blood oozing from the omental vessels after ovariotomy, occurred in my practice in September, 1863. The symptoms appeared in this case on the fourth day, and the injections were commenced on the seventh. The same kinds were used as in the preceding case—the solution of Liq. sodæ chlorinat., even ʒj. to ℥iv. of water sometimes; and it was found necessary to use them three times daily for twenty days, to keep the patient from sinking, then twice daily for twenty-one days, and once daily for thirty-three days more; making one hundred and thirty-five injections in all in seventy-

eight days.* I found it better to inject a large quantity rapidly and let it flow away immediately, usually injecting as much as the cavity would receive (one to two quarts at first). For the encouragement of others who may resort to this treatment, I should also add that it was persevered in, in the second case, for four weeks, and in the third for three weeks, before any amendment in the character of the decomposed fluid could be perceived.

In the last two cases I also administered the sulph. of quinine (one and a half to two grains every four to six hours), and the Liq. sodæ chlorinatæ (six drops, diluted, every four hours). The latter was used instead of the quinine whenever the tongue became brown or darker, and the breath offensive, and when the fluid removed became more fœtid for a day or two. The beneficial effects of both these remedies were very remarkable; and if discontinued for a few hours, the patient invariably got worse till they were again resumed.

I cannot doubt that the three patients above mentioned would have succumbed, had I not resorted to the treatment described. And it seems to me important to have demonstrated the practicability of thus rescuing a considerable proportion, I trust, from a condition which has hitherto destroyed over one-sixth of all who die after ovariotomy. A fair state of the general health is certainly not essential to its success; since two of the three patients were very much more reduced in strength before the operation, than I have indicated as conducing to a favorable result. (Page 55.)

Conclusions.

1. The success of ovariotomy in England and in the United States, unquestionably entitles it to be recognised as a legitimate surgical operation; the average of cures from all the operations being 61⅓ per cent.,† and of experienced operators 66⅔† per cent.; while some of the latter have recently saved more than 82 per cent. of their patients.

2. Ovariotomy is the only curative treatment in all cases of solid and polycystic ovarian tumors; and in cases of monocystic

* It having been omitted on four days during the last ten of the seventy-eight.
† *I. e.* 66 per ct. during the last four years; and 57.74 per ct. previously.

also, provided iodine injections have failed, or provided circumstances forbid both them, and the tapping per vaginam and leaving the canula *in situ.*

3. Certain conditions of the patient are favorable, and others are unfavorable to the operation; while others still entirely forbid it. These may now be definitively appreciated and stated. (See page 65.)

4. The incision should give an opening into the peritoneal cavity of only two or three inches to begin with, to be afterwards extended only as absolutely required, though in some cases to even twelve or fourteen inches.

5. Adhesions, however firm and extensive, do not forbid the operation, especially if parietal or omental. If visceral, they are a far more serious complication, and the adherent portion should sometimes be cut out of the sac and left in its contact with the viscus.

6. The objection to ligating the pedicle and leaving it in the peritoneal cavity, on the ground that the ligature produces a slough to be absorbed and produce septicæmia, is not sustained by facts. On the other hand, there are demonstrated objections to the clamp. The ligature is preferable to the latter; but it will probably soon be demonstrated that the best method of managing the pedicle is to ligate it, cut the ligatures close, and then entirely close the incision.

7. All fluid should be carefully and thoroughly removed from the peritoneal cavity by soft sponges, before the incision is closed.

8. The peritoneum should be included by the sutures in closing the incision.

9. The results of ovariotomy are very often determined far less by the operation itself than by the after-treatment, which should always be assumed by the operator himself.

10. Just enough of opiates should be given after the operation to control pain and restlessness and secure the proper amount of sleep—and *no more.*

11. If septicæmia occurs, the peritoneal cavity should be washed out by injections of warm water, or by a solution of the Liq. sodæ chlorinatæ, as required; this operation to be repeated from one to three times daily, as long as the fluid returns with an odor of decomposition.

THE INDUCTION OF PREMATURE LABOR.

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The justifiability of inducing premature labor in well-selected cases, is now admitted by the profession; and the history of the operation is accessible to all. The clinical difficulties, however, that surround the selection of the proper case for the operation, and the proper time and methods for its performance, are by no means surmounted. Some of these are insuperable, and must always be left to the tact, skill, and judgment of the consultation; while some have been, and others will be, resolved by clinical observation. My design in submitting this paper to the Academy is to contribute the results of my clinical experience in these cases, in the hope of eliciting similar contributions from others; for time has shown that the interests of our patients and of the profession are best subserved by free discussion of these questions in our medical societies, where alone the unrecorded experience and convictions of busy men can be reached. Before considering the indications for the performance of an elective operation, it is best to study the feasibility and the dangers of the operation itself; and, in accordance with the plan of this paper, I shall contribute such facts for the elucidation of these questions as my clinical experience affords; ignoring those which my judgment has led me, thus far, to disregard. The induction of premature labor demands that the maternal passages and the cervix uteri should be fully dilatable, and that the uterine forces should be efficient. Should time be an important element in the delivery, the latter may be supplied in great measure by operative interference with the hand or with

instruments. The rapidity with which labor may be induced is in direct ratio to the degree of dilatability of the cervix uteri at the commencement of the operation, and to the susceptibility of the uterus to contraction. To bring on labor in close imitation of nature, it is not only necessary that the cervix should be fully dilated or dilatable. This may be brought about, and maintained to the fullest extent, without the slightest uterine contraction in exceptional cases; just as the membranes may be ruptured, and the liquor amnii allowed to escape, with a similar indifference on the part of the uterus to contraction. The drain of waters may even continue for two months or more, and measures yet be necessary to bring on labor. Men who are ignorant of these conditions, or who have met with one or two prompt successes, are liable to make a faulty prognosis as to the time demanded in the next operation.

What measures are the most efficient in procuring full dilatability of the cervix uteri? I prefer to fix the mind on the dilatable condition of the cervix uteri, rather than on the degree of dilatation; because it is certain that many women are allowed to die when the cervix uteri is fully dilatable, while the consultation is anxiously waiting for it to be fully dilated. This distinction was early impressed on my mind in a case of placenta prævia which I witnessed abroad, where the degree of dilatation of the cervix was examined into several times during the day, and attempts at delivery negatived by the fact that there was no further dilatation. At last the moribund condition of the patient demanded the attempt, at least, at version; when the same operator delivered with ease, and exclaimed, as his fingers readily dilated the cervix, "It yields like wet paper." I have had many similar clinical experiences, and am convinced that the dilatable condition of the cervix is that really required for delivery. My memoranda furnish me with notes of many cases where I have been called to an os which had not changed in size, despite the use of the best remedies; when, in an instant, with my fingers, I have stripped it over the child's head without any difficulty. This fact has its application through the entire range of obste-

tric cases; and while its recognition may facilitate a natural labor, and render possible an instrumental or a manual delivery otherwise impossible, it finds nowhere a more important application than in these cases where labor is induced for immediate danger to the mother or child, and where life depends on the rapidity with which the end is reached.

The choice of means for dilating the cervix will vary with the degree of dilatation, and the facility with which the os uteri can be reached. In extreme cases of pelvic deformity, this latter contingency may be very important. We may, however, find the cervix well down in the pelvis, the os quite dilated, the head distinctly recognisable, and susceptible of ballottement through the unruptured membranes. Such conditions are not infrequent in multiparæ towards the end of gestation, and furnish the greatest facilities for the manœuvres required. We may find great varieties of direction of the cervix, and great differences in the calibre; and we may sometimes find the cervix so dilated as to resemble the amniotic pouch, and yet the small dimpled os uteri scarcely susceptible of recognition. Two such cases have occurred to me. The first fell to my care in the Dublin Lying-in Hospital. I encouraged the woman to bear down strongly, and anticipated a speedy delivery. This not occurring, and Dr. Shekleton visiting the ward, I suggested to him the advisability of rupturing the membranes, when he recognised the peculiarities of the case, and called my attention to a little dimple which represented the os uteri. In that case the first commencement of dilatation was brought about by the introduction of the end of a small catheter. The second case occurred in my private practice, about three years ago. My first impression, on making an examination, was that of gratification at the advance of the labor, but on searching for the rim of the cervix, I recognised the character of the case. The first dilatation of this os uteri could only be effected by the smallest-sized sponge-tent that we use in a contracted and unimpregnated cervix. Fearing that this little tent might slip within the uterine cavity, I attached it by a string to a body bandage. The rest of the labor was natural, after dilatation had been effected. The patient was a multipara who had been long under treatment

for chronic metritis, before this pregnancy, by Henry Bennett, and others.

The measures on which I rely for dilating the cervix, are, the douche; Barnes's dilators; tents of sponge, or sea-tangle, in exceptional cases only; and manual dilatation with or without the aid of forceps. Anæsthetics are uncertain.

It is my belief that the first case of induction of labor by the douche, in accordance with Kiwisch's plan, and the first case of induction of labor by Barnes's dilators, ever performed in this country, were performed by me. The first occurred in December, 1853; and the second in June, 1863. I have caused Barnes's dilators to be made in this city, through Messrs. Wade & Ford, with the addition of a stop-cock to each tube attached to the dilator.

So highly do I prize these methods, after an extensive trial, that I can scarcely believe that it will ever be necessary for me to incise the cervix of a parturient woman again. This has, however, occurred in my experience heretofore in three published cases; two of fatal albuminuria, and one of rigid os in a dwarf, which was published by my friend and colleague Prof. Taylor. They are, however, unavailing in overcoming one form of unattainable rigidity of the uterus above the cervix uteri. This contingency is fortunately rare, and has only been encountered by me on three occasions: one in the Lying-in Asylum, one in Bellevue, and one in consultation with Dr. Bishop. In all, the cause of delay could be recognised by others, and all demanded the greatest efforts for delivery. Chloroform was ineffectual; and in Dr. Bishop's case, I could only deliver with the purchase afforded by Scanzoni's cephalotribe. In that case, after delivery of the child, the constriction formed an imperfect hour-glass contraction below the retained placenta.

It may happen, then, that in exceptional cases a small sponge-tent may be necessary to commence with in cases of very contracted os; or in an os uteri rendered inaccessible by its position, or by the behavior or condition of the patient. Such a condition happened to me in a multipara with Bright's disease, hemiplegic and comatose, where I commenced the induction of labor with one of Mason's very largest sponge-tents. This

effected the dilatation, but the patient was moribund; and although I promptly delivered by version through the dilatation which I had effected, the child had died with the mother. In another case, where I induced labor in a multipara at seven months, for anæmia from a continued drain of liquor amnii, mixed occasionally with blood, which had lasted for seven weeks, a sponge-tent of the very largest size was selected, which rapidly imbibed the moisture, and brought on the labor without further assistance within eight hours. The child cried feebly, but did not survive. The mother did well. A multipara with placenta prævia, whom I saw in consultation with Dr. Bishop, in July, 1859, owed her life to an accurately fitted sponge-tent and tamponed vagina. The cervix was so singularly rigid, and the condition of the patient so low, that delivery was imperative, even though she died during the operation. Having previously separated as much of the placenta as two fingers could reach, with the tips of the fingers through the rigid cervix, aided by external manipulation and pressure through the abdominal wall, I reached a foot, and delivered. The mother did well. In such a case, where rigidity complicated hæmorrhage, and Barnes's dilators could be introduced, they would now command my preference; as they did in a case of a multipara seen by me, in consultation with Drs. Pulling and Wilson, on the 20th of March, 1863. In this multipara most dangerous flooding from placenta prævia had placed the patient's life in peril, while the diameter of the os uteri would measure less than one and a half inches, and was entirely undilatable. The dilators here acted delightfully, and in two hours' time uterine contractions had been brought on, the cervix would permit version, and there had been no hæmorrhage. The fœtal heart had become inaudible, and delivery was imperative, in the hope that it might still beat faintly. Introducing the fingers of my hand fully within the cervix, while the head was steadied at the pelvic brim by Dr. Pulling, I introduced the blades quite within the cervix, locked them, and drew the head into the world. The cervix came down before the head, instead of dilating, and I feared that incision would become necessary; but, by pressing strongly upwards on the anterior lip while I drew, it gradually allowed the head

to pass. The child was still-born—the mother did well. Such cases as these, where we have to bring on labor with great rapidity and deliver promptly, for danger to one or both lives, may demand, without delay, all the resources of our art.

It becomes, therefore, a very important question to determine not only the amount of dilatation and the degree of dilatability which will admit the passage of the head by the unaided uterine forces, but the extent of dilatability which will justify operative interference. If the child be certainly dead, and delivery imperative for the mother's sake, perforation of the head, and withdrawal by the crotchet, or Simpson's cranioclast, will demand less room than version or forceps. The varying size of the child's head, and its relations to the pelvis, so modify the conditions necessary for delivery, that rules can scarcely be satisfactorily laid down regarding the precise measurement of the diameter of the cervix which may be demanded. But the practical point to which my experience leads is, the preference which I would feel for forceps over version in such conditions as obtained in the case of placenta prævia, to which I have referred, when the child is believed to be living. I do not fail to appreciate the great advantages of version by the method prominently brought forward by Mattei, who does not receive the credit which he deserves in England. I have taught the principles which he advocates since the appearance of his work in 1855, and have published them in the New York *Medical Press* for 1859, in a clinical lecture delivered in the College of Physicians and Surgeons, when I was Lecturer Adjunct to Professor Gilman. The case of placenta prævia seen with Dr. Bishop, which I have just narrated, is an illustration of the application of those principles, before Braxton Hicks had so well defined the rules drawn from the practice of Wigand, Mattei, and others, which are destined to exert so powerful an influence in diminishing the mortality to the mother in version. But the risks of mortality to the child would not be influenced in a case of version where the impediment to delivery would rest chiefly with a rigid cervix uteri; on the contrary, the risks of departure of the chin from the breast, which attend every case of version, would be especially troublesome here. Undoubtedly no positive rules can

ever be laid down in such a case. The choice between forceps and version is preëminently elective, and to be decided by varying contingencies in special cases, and the peculiar fitness of the individual operator for one or the other operation. There are some fanatics, like Dr. Figg, in Great Britain, and like another practitioner referred to by Gardien in his great work, who clamor for version in nearly all cases, as diminishing the risks of natural labor; but they will never create a school. My reasons for alluding here to my preference for forceps in cases where there is sufficient dilatability to introduce a slender pair of blades within the cervix, so as to seize the head, which is retained within, are founded upon my convictions that interference is too often withheld in such cases by competent men, on account of the recognised principle that the head must at least be passing well through the cervix before forceps are applicable, and upon my convictions that such an operation affords the best chance for prompt and safe delivery. Undoubtedly an unskilful man should seriously hesitate in such a contingency, and most undoubtedly large and clumsy forceps should not be used; but a timely resort to interference, in cases where the life of mother or child is at stake, by a skilful man with proper instruments, will extend the usefulness of this operative procedure over a wider range than is yet generally appreciated. From my memoranda I present three other cases illustrative of these facts.

In one, seen with Dr. Bishop in 1862, a primipara aged nineteen, was seized with puerperal eclampsia, dependent on albuminuria, without intervals of consciousness. Dr. Bishop promptly commenced to dilate a rigid cervix, and bring on labor with the douche. When I saw the patient with him, just enough dilatability existed to admit the application of my forceps within the cervix. To draw the head through the unyielding cervix rendered it necessary that the blades should be brought together as closely as justifiable; but delivery was effected of a living child. The mother had many convulsions after her confinement, and did not recover consciousness for twenty-four hours subsequently. The child died two days afterwards, having seemed to do well in the interval. No post-mortem could be obtained. Here, then, was one of the most

dangerous cases of puerperal eclampsia which can be presented, in which such an operation permitted a prompt delivery with due regard to the safety of mother and child. For, what were the chances of that child's life if allowed to remain subject to such influences as overshadowed it in utero?

In another case the patient was a multipara, aged forty, with puerperal eclampsia associated with albuminuria, without consciousness between the convulsions; nor, indeed, did consciousness ever return during the twelve hours between her delivery and her death. Dr. Maxwell had succeeded with the douche in procuring a certain amount of dilatation of the cervix. The fœtal heart was beating—the mother's condition was dangerous in the extreme. Drs. Thos. F. Cock and W. H. Van Buren were also in consultation. In the judgment of us all, the choice of operative procedures lay between the perforator and the forceps; nor was much hope felt that forceps could possibly be applied. With the permission of all, I carried my forceps entirely within the partially dilated cervix, and succeeded in locking them upon the fœtal head, after which I dilated the cervix by drawing the head steadily and firmly upon it with the forceps, and delivered a living child. This child was spoon-fed, and died when it was a week old, from nothing connected with the mode of delivery.

The principle which I have advocated, of fixing the mind on the dilatability of the cervix, finds its application in many cases where delay is too dangerous to be submitted to without the most conclusive proof of its necessity. Thus I have published in the New York *Journal of Medicine* for September, 1857, a case of puerperal eclampsia in a multipara, with utter unconsciousness in the intervals. The fœtal heart was beating, the os high up, and allowed the introduction of only two fingers. Within two hours, by manual efforts alone, I had so dilated the cervix as to admit the forceps, when I delivered a living male child, which subsequently did well. The mother had more convulsions on the day after delivery, and could not be roused until the second day. She subsequently did well, but could never recollect some incidents which had happened before her confinement.

We may now consider the best method of employing the means

I have recommended for dilating the cervix. Reserving the sponge-tent and manual or instrumental dilatation for such conditions as the limits of this paper have already allowed me to mention, the use of the douche and Barnes's dilators demands further remarks. The one is always attainable; the other demands a special instrument, which is expensive, and very liable to disarrangment. Yet there is no escape from the conviction that no man is fully prepared for the contingencies which attend the induction of labor and rapid dilatation of the cervix uteri, who is unprovided with this, or a similar instrument. The dilatation of the cervix by fluid pressure applied from below, has taken rank in midwifery as a fixed and trusty procedure, based on the closest imitation of nature's laws; and the names of Arnot, Keiller, Jardine Murray, the younger Storer, St. Tarnier, and Barnes, are indissolubly connected therewith. But the great merit of making the method popular and practicable rests with Dr. Barnes of London, who has the same relations to this procedure that Morton bears to the introduction of ether as an anæsthetic. It is greatly to the credit of the learned and indefatigable Dr. Barnes, that he presents both his claims and the method so frankly, modestly, and completely. Still, as the douche is an earlier method, and one for which circumstances will always permit a wider application, its advantages are entitled to the first consideration. Believing that all can be accomplished by a proper use of Kiwisch's plan which can be accomplished by Cohen's, without any of the risks attending the latter procedure, I have never used the latter, and shall only speak of the former.

The douche cannot be used effectually unless the stream be directed steadily against and within the inner wall of the os uteri. A vaginal douche would be too uncertain in its action. Silbert, in his monograph, describes the inefficacy of the douche used as a vaginal injection until the stream was properly directed against the os uteri. It has occurred to me on several occasions to direct, in the practice of others, the use of the douche for the purpose of relaxing a rigid os and cervix uteri, and to find it ineffectual until similar precautions were used. In directing the stream against and within the os, the stream should be played against the inner wall of the cervix; and it

is to be remembered that an accumulation within the uterus is liable to change the position of the child, or to produce perhaps serious dangers to the mother. All risk of this kind is obviated if care be taken to see that the return of water from the vagina is about equal to that injected. I prefer always to use water of a temperature most agreeable to the patient, and to inject one or two gallons at once with an ordinary Davidson's syringe, taking care to keep the point of the forefinger constantly a little in advance of the nozzle, so as to appreciate the direction of the stream. I have memoranda of over thirty cases in which I have used the douche in cases of rigidity of the cervix interfering with delivery, and in cases of the induction of premature labor. These memoranda by no means represent my experience in the treatment of rigidity alone. It has very rarely failed to do excellent service, and in some cases has procured such dilatability as to allow of immediate manual dilatation, even where there had been no apparent progress. Used in the manner which I have recommended, with the patient on her back, as we place her for forceps, I have never met with any evil results from its use. It has failed me; it has demanded frequent repetitions and delay, but it has proved harmless to mother and child; and I would no more dread to use it with such precautions, than an ordinary vaginal injection.

Excluding those cases in which this agent has been used for immediate danger to the mother and child, as in the cases of convulsions and others, some of which are alluded to in this paper, I have memoranda of eight cases of premature labor, induced by design and appointment, with this agent alone, or in combination with others. In four of them no other means were resorted to.

In the first case, where labor was induced for pelvic deformity, one douche was given at 11 P.M., December 16, 1863. December 17th, 9 A.M.—The vagina and the os had commenced to relax. At 2 P.M., I repeated the injection. Uterine contractions commencing, os softened, vagina much relaxed. December 18th, 2 A.M., I went to repeat the injection; but labor had set in, and the child was delivered at five A.M. Thus two douches and thirty hours sufficed.

In the second case, labor was induced for deformity, with great anterior obliquity of the uterus. 1st douche, 3 P.M., March 22d, 1857; 10 P.M., repeated the douche. The patient was so restive that I brought her under the influence of chloroform before commencing. March 23d, 7 A.M.—Injection repeated. Labor had commenced. Cervix thinner; vagina cool and relaxing. 9 P.M.—Labor terminated naturally. Three douches in all. Thirty-one hours.

Third Case.—Labor was here induced for uncontrollable vomiting in pregnancy. It would have been induced before, if I could have obtained the consent of the consultation. When it was commenced, her condition was extremely critical, almost exhausted. Pulse 104, small and feeble. February 16, 1858, 10.45 P.M., 1st douche. 17th, 3 A.M.—Repeated, and followed by hot-air bath, as she complained of chilliness. 9 A.M.—Uterine contractions commencing. 2.45 P.M.—Child born; two douches, sixteen hours.

This patient continued to sink, and died during the night. The child was born living, but did not live many hours. On the autopsy the mother's liver was found to have undergone serious changes.

Whether or not this patient's life might have been preserved with such pathological conditions as obtained in the liver, may be questioned; but the case affords a threefold illustration of the great facility with which labor may often be induced in these cases; of the subsequent necessity for an operation, which may then have been deferred too long; and of the dangers which attend the obstinate vomiting of pregnancy. There can be no doubt of the fact, that the tendency on the part of the profession is to hope too strongly in some of these cases for the relief which so generally comes with time, or may be attributed to some of the therapeutic agents which we marshal against the complication. The mortality from this cause, which is recognised whenever practical men contribute the results of their experience to medical societies, proves the accuracy of this statement. While no one can be more averse than I to an unwise interference in a complication so generally tending to recovery, I yet strongly deprecate the delays which only allow the benefits of the operation to a

moribund woman. Here, as in the Cesarean section, and in all measures for operative delivery, the axiom of Professor Miller applies: "Meddlesome midwifery is bad; but a shilly-shally midwifery is worse." In the course of a debate on the paper of our late Fellow, Dr. Winchell, I expressed my views regarding the induction of abortion in these cases to which the same principles apply. Scylla and Charybdis often stand on either side. Cool, experienced, conscientious pilots are needed.

Fourth Case.—In this patient labor was induced in the second pregnancy, on account of the existence of albuminuria, with low specific gravity, shrivelled degenerate renal epithelium, and numerous pale transparent casts, a few of which were slightly granular, with granular epithelium attached. In the first confinement three years before, there had been convulsions, with albuminuria, which had slowly and very reluctantly yielded to the most careful treatment. I decided to induce the labor for these reasons:—

1. The child was fully viable and living.
2. It was established incontrovertibly that the kidneys had never entirely rallied from the albuminuria in the first confinement; that they now presented evidences of grave disease; and that there was sound reason for believing that these symptoms, which had baffled so much care already, would increase in direct ratio to the duration of gestation.
3. That in addition to the immediate risks to mother and child from the effects of the albuminuria, there were the greatly increased risks to her future health from the further persistence of the causes which had already proved so hurtful in her first labor, and which were likely to prove so much more serious now, when the kidneys were weakened by previous disease.
4. That the operation was not dangerous.

The first douche was given on the 30th of July, 1862, at 5 P.M. At this time the os was high up, and far back; not dilatable; and barely admitting one finger. The vagina was not at all relaxed, and rather dry. I injected about five-sixths of a large pitcherful of warm water. Before the conclusion of this injection, the os was sufficiently dilated to allow me to corroborate through the os the diagnosis of the presentation of a head,

which I had previously been able to make through the cervix. A suture had been distinguishable also in that way. Before leaving the patient, I drew down the os so as to bring it in a position more favorable for the douche, and for the descent of the membranes. Labor pains set in, and prevented her from sleeping.

July 31, 5 A.M.—Os uteri larger than a dollar, and dilatable; membranes unruptured; right thigh now recognised across the os uteri.

Another douche. Gave chloroform for great restlessness (always an alarming symptom in uræmia). I sent for Dr. Thomas, who kept up the anæsthesia. I then tried to turn by external manipulation, but could only succeed partially, so I replaced the thigh by pressure through the uterine wall; gave a third and small douche; after which I fully dilated the os with my fingers, and allowed the membranes to rupture spontaneously. This they did in an hour, when the right knee came over the vulva, and I delivered her manually of a living male child without delay. The cord was three times around the neck, which may have impeded my efforts at version by external manipulation. The child seemed at first indisposed to breathe, but after much less than the customary manipulation in these cases, Dr. Thomas and I considered that he would do well. Some two hours afterwards symptoms of cyanosis manifested themselves. Some comparative dulness could be recognised over the upper and anterior part of the left lung. Dr. Thomas, Jacobi, and I did all we could; but the child died in the afternoon. Some atelectasis beneath the site of dulness was the only abnormal condition that we could find, even the blood having been examined for urea.

Thus, in this case, three douches, with manual dilatation, allowed me to terminate the labor readily in sixteen hours from the time when every condition of the uterus and vagina was as unlike labor as possible in pregnancy.

The facility with which the whole procedure was accomplished was very satisfactory, despite rare obstetric contingencies; but the loss of the child has always been a source of great regret to me. He was so well formed, and of such size, that the debility which we fear in premature children did not appa-

rently complicate the case. The same result, after an ordinary labor, would scarcely occasion remark; but in many of these cases of premature labor, where the operation is elective, a man takes heavy responsibility if success does not crown his efforts, even when the decision has been well taken; and this argument is one of those which naturally tend to deprive our patients in many cases of the advantages of a timely resort to the operation.

In these cases of albuminuria, and in many other cases, the operation, while always elective, yet differs when undertaken at a fixed time, for the prevention of well recognised risks, or when undertaken after these dangers have suddenly presented themselves. The responsibility is always heavy. For my own part, for reasons alleged heretofore by me to this Academy, and which could be still further enforced, I consider that the greatest ratio of risk falls to the multipara. In both classes, however, the best prophylactic measures should be used before resorting to the induction of labor. But when these fail, and the urine, the nervous system, or other organs, show that undiminished or increasing danger still throws its gloomy shadow over the case, then the consultation must seriously consider the alternative of premature labor.

It has occurred to me to recommend its constant consideration during the progress of the pregnancy, as in the case of a multipara seen with Dr. C. L. Mitchell, and to have the symptoms abate under treatment, and a premature dead child expelled without interference. It has occurred to me to consider it anxiously from day to day, after violent puerperal eclampsia had occurred in the same pregnancy, and yet to witness the happy success of hygienic and therapeutic treatment alone. It has occurred to me to consider it anxiously, and to decide against it, only to have the sorrow of witnessing the sudden death of the undelivered woman in a convulsion not apparently more severe than the others. It has occurred to me to be summoned in haste to a similar case, and to remove rapidly a dead child by the post-mortem Cæsarean section. There is no escape from anxiety and harassing responsibility; but my convictions are, that in the majority of these bad cases, endangered by the results of albuminuria, and suffering from convulsions or other

of its consequences, mother and child will have the best chance by the judicious induction or rapid completion of the labor, by the methods which I am considering. And it has occurred to me to witness the beneficial effects of the douche in a case seen with Drs. Peugnet and Nordquist, where albuminuria and fearful convulsions occurred in a primipara with a non-viable child, about the third month; and where we succeeded in removing the ovum, and ultimately in restoring the patient to health. But of all these cases in which the induction of labor demands the most serious consideration, multiparæ, and especially multiparæ who have previously suffered from puerperal albuminuria and its consequences, claim preëminently the timely advantages of this operation, when our best prophylactic and therapeutic measures do not succeed. In such cases, calmly and competently studied, the induction of premature labor offers most beneficent results, and challenges the serious consideration of every conscientious practitioner.

The limits of this paper forbid further consideration of this point; and I pass to the methods for inducing labor which I have employed in other cases than those in which the douche or manual dilatation or the sponge-tent alone sufficed.

Notwithstanding the promptness and facility with which labor was brought on in the series of cases just detailed, by the douche alone, it is yet liable to fail, under very exceptional circumstances, in bringing on uterine contractions, and still more exceptionally in procuring dilatation of the cervix. Moreover, there are frequent occasions in which time is such an element in the success of a case, that we grudge even the risk of delay, and are bound in duty to select those measures which reduce the risk to a minimum. It is just, however, to say that, with every appliance, no one can anticipate a fairer share of success in the rapid induction of premature labor than in the series of cases just narrated. And it is to be remembered, that whenever the cervix is fully dilatable by any means, the operative measures at our command always allow an immediate termination of the labor, if the conditions justify the addition of risk from the complications attending operative interference. Such risks differ in the practice of different men; but the great end to be kept in view, in the perfection of

the processes for the induction of premature labor, is their assimilation to the phenomena of natural labor without further operative measures.

As an example of these delays, I offer the following case, in which I induced labor for deformity of the conjugate diameter of the brim, in Bellevue Hospital.

At about the time which I had appointed for the induction of labor, October 15th, 1861, the patient was ascending a stairway when the membranes ruptured spontaneously, though no very great amount of liquor amnii escaped. On making a vaginal examination, the os and cervix uteri were found soft and slightly dilated, while the vagina was plentifully lubricated with mucus. Labor-like pains occurred at regular intervals until the next morning.

This was a most encouraging state of things, and one which promised every indication of successful delivery without interference. It has occurred to me in four or five cases to have labor come on at the time which I had decided upon for its induction, and sometimes on the very day. Thus, Dr. Bolton and I had agreed on the day, in the case of a multipara, who had suffered from uræmic eclampsia a short time before, while advanced in pregnancy; and on the day fixed she was spontaneously delivered of a premature dead child. In another case I appointed the day for the induction in a rachitic primipara, with diminished antero-posterior diameter of the brim, and great curvature of the spine (lordosis, kyphosis, and scoliosis). The outlet was well formed, and the pelvis shallow. On the night before the day for the operation she fell in labor, and gave birth naturally to a living child.

It is my belief that most men of large experience have met with similar instances. Would that such results might always follow! But in the case which we are now considering, the pains ceased on Thursday morning (17th), and at half-past eleven I injected two gallons of warm water. Uterine contractions took place almost immediately, and the os became softer and more expanded. The pains continued, but ceased towards morning of the 18th. The os was found at my visit almost entirely closed. Repeated the douche with the same effect on the pains and the dilatation, but during the evening

the os returned to its former contraction. Allowed to sleep during the night. 19th.—No pains; bowels have moved. Third douche.—Os dilated to the size of a dollar; no pains. A few days not making a great amount of difference, I allowed Sunday to pass. 21st.—Patient in good condition. Fourth douche, with relaxation, but no pains. 22d.—Patient has not slept well; is nervous, hysterical, and complains of rigors. 23d.—Patient's condition good; introduced a large-sized sponge-tent, which I allowed to remain in for twelve hours. 24th.—On removing the tent, the os was fully enough dilated for delivery, though there had been but few uterine contractions. A galvanic current was then used several times during the day, with one pole over the spine, and the other over the abdomen, and within the vagina alternately; occasional uterine contractions, but no effect. 25th.—Disappointed with the results obtained from some discharge of waters, douche, sponge-tent, and galvanism; prepared a part of a No. 8 elastic male catheter, three inches and a half long, with a long string attached to facilitate its removal; and having introduced this catheter between the uterus and the membranes anteriorly, allowed it to remain *in situ* for fifteen hours. True and good uterine contractions then set in at long intervals until the 27th, at 5 P.M., when strong labor-like pains commenced, and at 10 P.M. the head had passed through the fully dilated os into the excavation. There had been no cause of delay from any relations of the head to the pelvis.

Some of the details and considerations suggested by this case will be presented in another part of this paper. The point now for remark is the great sluggishness of the uterus, and its relations to the difficulties that attend the induction of labor. If the case had demanded immediate delivery, I could, of course, have delivered by rupturing the membranes, and attempting a formidable forceps operation entirely above the pelvic brim, in a contracted diameter; or I could have turned and brought down the feet—a preferable procedure in this particular case. But it seems to me that I treated the case better in managing it as I did. Yet my further clinical experience, based on the observations of this case, has taught me to interfere more actively when such sluggishness threatens to delay

the labor. In this case, where full dilatation of the cervix was so often accomplished, and where the sponge-tent failed, and the elastic catheter was so slowly effectual, it is very evident that Barnes's dilators would not have been more serviceable. My subsequent experience has led me to consider the early introduction of a flexible catheter, left *in situ*, with preliminary separation of the membranes, as a valuable method. One word in regard to separation of membranes. It is probable that we all admit that one element in the rapid progress of a labor lies in the recognition, at an early stage, of the bag of membranes coming well within and through the os uteri. Early separation of the membranes favors this result, and acts as an additional stimulus to uterine contraction; but integrity of membranes, in my judgment, is a fact of the greatest value in the facilitation of the progress of the labor, and diminution of the risks from the labor to the mother and child. Hence this separation of the membranes is not to be the rule, and rarely to be performed before uterine contractions have set in, and the cervix uteri somewhat dilated. How differently, for instance, would I have been situated towards the case just detailed, if all, or most of the waters, had escaped at the commencement of labor.

Second Case.—Some of the principles just alluded to find their application here. The patient was a multipara, who in a second pregnancy had presented eclampsia. The urine, however, was but little affected, and diligent search had only rarely shown any albumen, and a cast or two. She became hemiplegic, and had choreic convulsions, with tendencies to cerebral congestions. She had been seen by Drs. Van Buren, Clark, Swift, and Echeverria, and, I believe, by others. Pregnant again, my opinion was asked, and given in favor of delivery at any time in which the mother's life became seriously compromised; but if care and treatment prevented this risk, then my opinion was decidedly in favor of bringing on labor at the eighth month, as I thought that she should in any event be spared the dangers and delays of the last month of pregnancy. Accordingly, this unfortunate woman went on with her pregnancy, and at the eighth month I commenced the induction of labor with Barnes's dilators on the 9th

of June, 1863—being, in my belief, the first case in which they have been employed for the induction of labor in this country. At 9 A.M., the os was high up, and reached with some difficulty. The finger, when forced through, could detect a cranial presentation. Cervix uteri thick and long. Managed to introduce the smallest-sized dilator. 12 M.—The first is nearly in the vagina; relaxation of the cervix and vagina has commenced. Introduced the second size, into which I could inject three syringefuls of water. 2 P.M.—This was slipping in the vagina. Pains have commenced; some show. 4 P.M.—No pains. Re-introduced the second size, and injected it with six syringefuls of water. 7 P.M.—Dilator found in the vagina, where it had probably been forced by some strong expulsive pains which had occurred at five o'clock, and had suddenly ceased. Os uteri now nearly fully dilated, though not absolutely dilatable. Slight pains recognised by abdominal manipulation, and by the finger pressed against the bag of waters, which was quite thin, would become tense with pain, and was commencing to protrude within the cervix uteri. Vagina relaxed and moist. I was fearful of rupturing the membranes if I re-introduced the dilator within the cervix again. Accordingly I introduced the largest size, fully dilated, within the vagina, and reinjected it about 10 P.M., when I left it in all night, during which she had some pains and some sleep.

June 10th, 5 A.M.—Os fully dilated, vagina relaxed, membranes now protruding; head not advanced; fœtal heart beating. I then introduced one-half of a No. 9 catheter, with a string attached, and left it *in situ* between the uterus and membranes. In the course of the day the labor terminated naturally. The child was living, and has done well. The mother is living, and has improved in health.

The principle which I advocated in this case, of allowing the pregnancy to go on in the interest of the child until fully viable, and of then bringing on labor in the interest of the mother, applies to a vast range of cases where diseases of the kidneys, heart and blood-vessels, lungs and respiratory apparatus, nervous system, or complications, as from the presence of abdominal tumors, motive a consultation for danger to mother and child. It applies also to cases of successive occurrences of fatty placentæ in different pregnancies.

Third Case.—Labor induced for deformity of the brim in the Lying-in Asylum, with the assistance of Dr. Sterling, Resident Physician, June 8th, 1865. 10½ P.M.—A tepid douche for twenty minutes. 9th, 8 A.M.—Os uteri sensibly dilated. Bowels being constipated, enema ordered, which produced two movements. 9½ A.M.—Second douche. 12¼ P.M.—Os dilatable to nearly the size of a Mexican dollar. Barnes's dilator, No. 2, was introduced and injected until she began to complain of pain. Cranial presentation previously recognised. 2¾ P.M.—Dilator readily removed; cervix still long, well dilatable; membranes distinct and distended. 3¼ P.M.—Largest-sized dilator introduced, and distended fully without complaint. 9 P.M.—Dilator removed by the hand. There have been pains, which have ceased entirely. They returned under digital examination, and these becoming pretty fair, and the membranes commencing to pass well down the cervix, decided not to introduce the dilator. 10th, 12½ A.M.—The pains have again markedly diminished, but return when the digital examination is made. I then introduced a piece of a No. 8 flexible male catheter between the membranes and the uterus, in front of the right sacro-iliac synchondrosis, and left it there. 6 A.M.—The pains have again subsided; but they had come on with sufficient violence to prevent the patient from sleeping. The catheter was in the vagina, and I replaced it anteriorly. 11½ A.M.—Pains have been moderately good; catheter *in situ;* membranes on a level with the os externum, in the intervals of a pain.

11th, 12½ A.M.—Almost continuous pain; catheter withdrawn, and not replaced. Towards morning some chloroform given in moderate quantities for relief from the force of the pains. 8½ A.M.—Membranes which have touched the floor of the pelvis have been allowed to rupture spontaneously. I aided the entrance of the head through the contracted brim by pressure from above with some success.

11¼ A.M.—Child born with three coils of funis very tightly around its neck, and very feeble. Dr. Sterling's care soon revived it; and the child, a vigorous boy, has since done well, without a single unfavorable symptom, as has the mother.

Case Fourth.—Labor induced for contracted brim, in consultation with Dr. Sayre. First douche, November 28th, 1865,

11 P.M.; and then smallest-sized dilator introduced. This soon slipped out. 29th, 9 A.M.—Os admitted two fingers. Delayed second douche until 11 A.M., at her request. After that, introduced second dilator, and distended it to the extent of her ability to endure it. In three-quarters of an hour this slipped out. 1 P.M.—Pains have commenced. Os more dilated. Third douche. Decided not to re-introduce the dilator, because I recognised a hand to the right side of the head, within the membranes; and because the latter were beginning to pass well into the cervix, and were thin and tense. Pushed up the hand, and pressed on the head meanwhile through the abdomen. 4 P.M.—Pains moderate; fourth douche. 5 P.M.—Introduced part of a No. 8 elastic male catheter on the left side. Catheter expelled at 8 P.M., after vomiting, and replaced behind the right acetabulum, and this was expelled at half-past twelve. 30th, 9 A.M.—Has slept well, but has been awakened at half-hour intervals by pains. Introduced and distended largest-sized dilator, which was expelled in three-quarters of an hour. 11.30 A.M., fourth douche. Re-introduced catheter behind right acetabulum. 4 P.M.—Pains very good, and membranes pass slightly below the os. Catheter withdrawn. 12.30 P.M.—Child born—boy, and living. Has since done well. The membranes ruptured an hour before.

In a rare and terrible case of vomiting from pregnancy, coming on with such suddenness, violence, and prostration as to cost the patient her life in a few days, while yet the child was non-viable, Prof. Budd requested the advice of Prof. Barker and myself. Dr. Taylor had also seen the patient. Delivery gave her, in our judgment, her only and most remote chance. In that case, Barnes's dilators acted with their customary success, and the membranes ruptured at an early period. But the rapid collapse of the patient was such, that when dilatation had been sufficiently effected to admit of the removal of the child, the patient was so very low that we allowed her to die undelivered; as we thought that a slight hæmorrhage, or the mere shock of delivery, might shorten her few remaining moments. In the management of these overwhelming cases, combined with such great jactitation and impatience of advice as were presented here, these dilators surpass all other mea-

sures. A curious and unique observation was made here: the dilating cervix would suck the fiddle-shaped dilator towards the uterine cavity, instead of repelling it.

In a case which I saw with Dr. Hutchinson, the douche, venesection, chloroform, manual dilatation, and time, had failed to dilate the rigid os to more than the size of a dollar. In two hours the dilators procured a dilatation which permitted Dr. Hutchinson to introduce his hand and deliver. Many other cases of their efficiency and value could be drawn from my memoranda, and recollection of their value in cases of my own, and cases which I have seen in the practice of others; but it is best for me to sum up at once the advantages offered by these different procedures, and to assign them their clinical rank:—

1. *The douche* is always attainable, always serviceable, very often entirely efficient in itself; never painful or dangerous, when properly used, in cases where there are no conditions which prevent the risk of fatigue, delay, and some exposure which its use demands. These objections, and the risk of faulty direction of the stream, or admixture of air, in careless hands, are the main objections that can be raised against its use. I should not attach value, from my experience with it in localities where puerperal fever is liable to be bred and propagated, to any argument that it is liable to induce risks of metro-peritonitis. The only influence which it can possibly exert for evil on the child, is where its faulty use might change the position. And in considering this risk, we must not forget the recorded cases where such "culbute" has been observed without any recognised cause. I should not think of employing it in cases of placenta prævia or ante-partum hæmorrhage; nor should I in other cases apprehend that it would provoke hæmorrhage.

2. *Barnes's Dilators.*—These can always be employed with facility where two fingers can pass into the os; and are susceptible, in favorable cases, of earlier use. They are more widely useful than the douche, and are applicable in cases of hæmorrhage, jactitation, and insubordination, where the douche cannot be used. They are more certain, and can be relied on beyond any other measure to rapidly dilate the cervix, and allow the confident anticipation of a very rapid

operative delivery within a few hours. They combine facility of introduction, facility of retention, hydrostatic pressure, application of heat or cold, compatibility with frequent vaginal examinations, with comparative safety against internal uterine hæmorrhage.

They are liable to be drawn, in exceptional cases, into the uterus which has not lost its spheroidal form; to rupture the membranes too early; and possibly to induce change in the child's position. It has occurred to me that in cases where the funis has been replaced in utero while well pulsating, and a rigid cervix or other conditions interfere with delivery, these dilators, well distended, offer the best guarantee against a reproduction of the prolapse, while facilitating the prompt delivery.

It is probable that their use will always supersede that of sponge or other tents, except in such rare contingencies as I have described, and do away with the use of manual dilatation, which is always liable to be abused in inexperienced hands.

It is probable that the hosts of men who delight to attach their names to a serviceable invention, will yet improve these dilators so as to do away with some inconveniences in their introduction, and applicability to variations in the character of the cervix and the presentation, and render them less likely to rupture and wear out.

3. *Sponge, or other tents*, are valuable where the dilators cannot be procured and in rare cases where they cannot be introduced, and there are objections to the douche.

4. *Manual dilatation* is serviceable where the same conditions obtain, and especially where there coëxists great dilatability without dilatation—a fact only to be appreciated by trial.

5. *The introduction of a catheter between the membranes and the uterus* is justifiable whenever all other conditions for delivery are favorable, and where the contractions of the uterus have been imperfect or sluggish. Although theoretically a harsh procedure, it does not appear to increase risks of metritis; and is very effectual when left *in situ*. A renewal of the trials of very flexible materials which have been used in the history of the operation, will probably be advantageous.

6. *The membranes should be separated* when the cervix is commencing to dilate satisfactorily, if this does not involve too great a risk of rupture; as the early descent of the membranes, in a favorable presentation, facilitates the phenomena of natural labor.

7. *The electric and galvanic currents* demand further trial, and will probably yet enable us to dispense with other means of inviting uterine contractions, notwithstanding the discouragements which have so often attended their use.

8. *Puncture of the membranes* is to be avoided in all cases where the danger to the mother does not point to this procedure, as may happen in placenta prævia, and over-distension of the abdomen; or where the labor can be promptly completed.

9. *Medicines* are not specially indicated for the induction of premature labor. Their use is to be formulated on other considerations.

We pass, now, to the consideration of—

Deformity of the Pelvis.—Didactic works distinguish with great accuracy the varying influences exerted on the progress and termination of labor, by differences of a quarter of an inch in the measurement of a pelvic strait; and yet we often find that these very authors give unsatisfactory rules and tests for the recognition of these shades of difference.

Our clinical records show that the autopsy has proclaimed the error of one excellent man—as in the case of Villeneuve—who relied on external measurements, and who made the customary approximative deductions for the thickness of the soft parts of the bones, but was deceived by the unusual thinness of the sacrum. Most practical men, however, will be willing to appreciate the increased risk of this method of examination; but we find that the autopsy will also disclose the error of another first-class man—as in the case of Lehmann, reported by Rodenberg—who relied on internal measurement by the finger alone, and who was deceived by the unexpected difference between the diameters of the conjugata vera and the conjugata inclinata.

For this latter, and similar sources of error, the majority of men would be less prepared with excuses; for, perhaps,

by common consent, the finger of an educated obstetrician like Lehmann is considered to be the best pelvimeter. Indeed, but very few of the many ingenious instruments devised for this purpose, and so well illustrated by Lenoir, have been recommended by others than their inventors.

It is true that no measurement can be relied on that is not appreciated by the educated finger; but I deny that accurate measurements can be made by the hand alone of some of the pelvic diameters, and I claim that the skilled use of the instruments will confirm and enhance the accuracy of the measurement of others made by a competent man. They will never, however, supply the place of patient bedside education of the hand. In confirmation of these views, I point to the shadowy and unsatisfactory rules that are given for the measurement of the transverse and oblique diameters of the brim by the fingers alone. The measurement is so unsatisfactory, that those who rely on the method take comfort in reflecting that the chances of deformity in these diameters are less than in the antero-posterior. The uncertainty in this measurement, and the comparative infrequency in deformity of these diameters, have logically led to a neglect of their careful study, and thence to great evils in practice. May I not venture to state the following propositions?

I. The immense majority of practitioners who apprehend pelvic deformity in a given case, seek no further if they can not touch the promontory with their finger.

II. There are very few men who would be willing to record their measurement of a pelvis, where differences of a quarter of an inch might be tested by an autopsy, without desiring that large allowance should be made for liability to error.

Indeed the difficulties are so great, that we find Churchill endorsing with his approval a quotation from Velpeau in reference to the uncertainty of pelvimetry. When we pass from the text-books to the records of clinical cases, and the discussions of medical societies, we find, as might be anticipated, a larger latitude given to the liabilities to error, and painful illustrations both of cases where deformity has

not been recognised, and of others where imperative indications have been neglected.

The relations of pelvic deformities to the induction of premature labor, have prompted these prefatory considerations; but these are the great practical considerations with which we have to deal:

1. Are these deformities frequent?
2. Can they be accurately estimated?
3. What indications for treatment do they present?

1. *Are they frequent?* I believe that Dewees has truly signalized the great exemption of our countrywomen, and the fact that those we meet with exist in a large ratio among those who have immigrated here. But when we consider the influences at work among our native born poorer classes, and the enormous tide of immigration, we must be prepared to recognise that deformities of the pelvis have become an obstetric contingency which we are now called on to grapple with more frequently than even so recently as the time of Dewees.

My memoranda furnish me with twenty-two cases in which I have recognised the existence of pelvic deformity in the parturient woman. In all of these, one or more diameters have been diminished by pelvic contraction from rachitis, injury, or exostosis.

In five of these cases I have induced premature labor. All the mothers did well. Two of the children are now living. One was still-born from the complication of prolapsed funis with a footling presentation. One was born living, but died in four hours in convulsions which followed epistaxis to the extent of about one ounce. No instruments were used in either of these cases; but this child presented, when born, a depression of the frontal bone below the parietal, to the extent of half an inch. The autopsy detected extravasated blood beneath the arachnoid at the posterior part of the convex surface of the right hemisphere; clot the size of a gooseberry in the posterior horn of each lateral ventricle, and a smaller one in the left of one thalamus; blood extravasated at the base of the brain.

In the case of this primipara, labor was induced after con-

sultation with my colleagues, Drs. Metcalfe, Taylor, and Barker, mainly on account of posterior spinal curvature of the lower six dorsal vertebræ, and possibly of the first lumbar, with the accompanying distortion of the sternum and ribs. She was four and a half feet high, and I satisfied myself after delivery that the pelvis was contracted in one oblique diameter of the brim. There was also a loud systolic basic murmur transmitted downwards in the mesian line, but scarcely audible at the apex. When labor was induced, there was great anteversion of the uterus, from diminution of the space between the sternum and the pubes, which was not susceptible of reposition. The result of the case strengthens the arguments for the procedure; for the occiput presented anteriorly to the left, and the exaggerated obliquity drove the head into the brim at such an angle as to cause the depression by pressure, and the consequent extravasation.

In the third case the head had passed into the excavation below the contracted brim (three inches, ant. post.), when it was delivered during my absence by my colleague, Dr. Taylor, with forceps. The child gasped after delivery, and the heart beat, but it could not be resuscitated. In this case the autopsy showed great depression of the left fronto-parietal suture, although the mark of the forceps was on the right brow and behind the left ear in the opposite oblique diameter. On removing the calvarium, from six drachms to one ounce of fluid blood escaped. No extravasation into brain tissue or ventricles.

In the fourth case, a multipara, the child was born without instrumental aid; and though feeble at first, from having three coils of funis tightly around the neck, has done well. In this case the measurement with Lumley Earle's pelvimeter, after delivery, gave three and three-eighths of an inch of antero-posterior diameter of brim.

In the fifth case, a multipara, the child was born without instrumental aid, and has done well. In this case the antero-posterior diameter of the brim measures four inches, and the transverse the same; and the linea ileo-pectinea, from the pubes to the right ilium, is straighter than it should be.

Of these five cases, the first had been twice before delivered

with the perforator and crotchet; the second was a primipara; the third had been delivered in her first labor with the perforator; in the second, she had been delivered without assistance, after a lingering labor, of a dead child, and required eight or nine operations for a vesico-vaginal fistula, resulting from the continued pressure. The third labor was induced by me; and I am informed by my friend, Dr. H. S. Hewit, who summoned me to the first labor [in which she was also seen by Dr. Metcalfe]—who cured the vesico-vaginal fistula, and who still continues to the patient the advantages of his charitable care—that she has since been delivered by forceps of a small child, at term, after a protracted and natural labor. The fourth case had been previously twice delivered in the West, at term, by the perforator and crotchet; and in the fifth case difficulty had been experienced in the delivery of the child in the first labor, with the perforator.

Although I am indebted to my hospital advantages for a large part of this experience in pelvic deformities, it still warrants my conviction that these deformities are becoming more frequent in obstetric practice.

2. *Can they be accurately estimated?* It is my conviction that in the great majority of cases the measurements can be accurately estimated. Consequently I believe that we should retain the didactic rules regarding differences of a quarter of an inch, and that the profession should be stimulated to increased effort in the accurate recognition of the amount of deformity. In these measurements, as in the recognition of other physical signs, that man will have the greatest success who is most conscientious, skilled, and faithful in his examination, and who patiently examines all the diameters, and uses all methods to preclude sources of error. Those who know how hard it often is to determine relative shortening in a fractured thigh during the progress of treatment, will be prepared to appreciate the difficulties involved in the accurate measurement of the distance between certain points situated deeply within the body, which can never be seen except at an autopsy, and which are liable to such protean changes. Hence the liability to error is so great, that the best men must expect to

fail occasionally; but is this not true of all other diagnoses based on physical exploration? A man can only do his best, and in the event of error he will experience here, as in similar situations, the most lenient judgment from those who know most about the subject. The practical conclusion to be drawn from the liability to error is, that if the consultation should not feel warranted in relying firmly on the result of their examination, they should give the mother and the child the benefit of any doubt.

In looking over the memoranda of my cases, I find much vagueness in the accurate recognition of the character and extent of the deformity in many cases, and may have been mistaken in a certain proportion, although nearly all have been seen by others. But I have also the satisfaction of noting that, with time and labor, and the aid of pelvimeters, sources of error have been more and more excluded; while one case furnishes me with the evidence, drawn from the autopsy, that my ante-mortem and the post-mortem measurement of the conjugata vera only differed one-eighth of an inch; and this deformity was recognised in the hospital, after the woman was in labor, with Lumley Earle's pelvimeter.

It is my conviction that in the hands of men accustomed to manipulations within the pelvis, such results should be the rule and not the exception.

How, then, should these measurements be made? It has never occurred to me to be consulted regarding the obstetric diameters of the pelvis in an unmarried girl or in a married woman who had not been pregnant. If the opinion be sought during a pregnancy, while there is yet time for study of the case, at least two examinations are desirable before a positive opinion should be expressed. It is desirable that the bowels and the bladder should be thoroughly emptied, and tendencies to flatulence dissipated if possible. The patient should be clothed in a chemise, night-gown, light dressing-gown, drawers, and stockings, and so arranged on a bed or examining chair that she can be examined on her back and on her side, with her hips projecting beyond the edge of the bed. It may be useful to put her on her hands and knees, in order to lift the uterus and fœtus, as well as to study the pelvis. A preliminary digi-

tal examination had better be made while the patient is standing, as much can be learned in that way. If the patient prove very sensitive, timid, and restless, an anæsthetic should be administered. In one of my cases a thorough examination was rendered impossible until the close of the labor, by the behavior of the patient. With a docile, intelligent patient, in whom the examination produces no pain, we can do better without an anæsthetic. The measurement of the pubic arch and of the outlet is easily accomplished, the mobility of the coccyx readily appreciated; and then a careful study should be made of the spines of the ischia and of the whole anterior curve of the sacrum to the promontory. If the patient be not pregnant, or excessively stout, conjoined manipulation with both hands will here obviate all risk of error in the recognition of the promontory. But, if she be pregnant, the risks of mistaking an exostosis, a projection forwards of one or more of the bones of the sacrum, a slipping down of the lumbar vertebra, or even a markedly defined transverse line, for the promontory, are only to be avoided by very careful study. True, the diagnosis of deformity may thus be accurately estimated, and the nearer the projecting part to the outlet, the easier the recognition; but the diagnosis should aim at more than the recognition of the contracted diameter—it should take cognizance of the anatomical or pathological elements involved. No deformity of the sacrum should pass inspection until very firm pressure has proven that the suspected deformity is not due to fæcal accumulation. The extent and the hardness, as well as the durability of these, are truly remarkable. No accumulation of fluid should receive even serious attention, until the introduction of a long flexible male catheter has eliminated the possibility of its depending on distension of the bladder. No tumor, due apparently to projection, enlargement, or degeneration of any organ, should be allowed to escape a thorough examination, to determine whether it may not possibly be movable and susceptible of being pushed above the brim, brought externally, or shoved in front of one sacro-iliac synchondrosis; and whether it may not be partly fluid, and capable of being tapped. The length, concavity, and form of the sacrum, and the relations of the promontory to the pubes and to the transverse

diameter of the brim, having been estimated by the finger, or by all the fingers, or by the fingers and palm of the hand, or by the whole hand if necessary, or by two plumb lines, the size, depth, and internal conformation of the pubes next receive attention. Both hands should now be used conjointly, and after the pubes have been seized between the two, the linea ileo-pectinea should be appreciated by both hands if possible, or at least in this way a very good approximative appreciation of the anterior wall of the pelvis will be reached. The obturator foramina should also be studied. A final appreciation of the transverse diameter of the brim may now be made to the best of one's ability, before proceeding to the accurate measurement of the antero-posterior diameter. I have always found it necessary to satisfy myself several times that my finger was really touching the promontory of the sacrum before I could feel willing to trust to my measurement in this diameter; yet in this way, when examination of the pubes in the manner detailed has shown it to be normal, the deduction of a half-inch for the difference in the diameters will, in the great majority of cases, give an accurate result.

But it may be impossible to use the finger in the way ordinarily recommended, and yet there may be great deformity. An arc of parietal bone, with a tumid caput succedaneum, may prevent, and the growth of the caput may convince the unwary that the head is advancing; that consequently there can be no deformity, and that measurement is unnecessary. I have notes of a case in which a difference of opinion regarding deformity of the brim led to a regrettable delay, and where I delivered a child weighing six pounds, with forceps, which presented on delivery a marked depression, made by the promontory of the sacrum on one parietal bone, and a smaller depression opposite. The child lived for two days, and died from apoplectic effusion. The pelvis of the mother was shown to the Pathological Society. It measured three inches in the ant. post. diameter of the brim. The promontory was turned a little to the right, and was noticeably sharp.

Now, whenever the finger is prevented from passing satisfactorily in the place of the conjugata inclinata, the hand can only appreciate the deformity by placing the tips of separate

fingers on the promontory and behind the pubes, and in this way I deny that measurement can be as accurately made as with a pelvimeter. I do not deny that such an examination may not be made which may be confirmed by examination after delivery; but I regard the result as exceptional, and as a signal triumph over difficulties that can be more readily and reliably overcome by other measures.

In the measurement of the antero-posterior diameter of the brim, under the circumstances, Lumley Earle's pelvimeter has enabled me to come within an eighth of an inch of the post-mortem measurement, as has been already stated; and if the instrument sent me from London had been made according to the drawing of the original instrument, it is my belief that this trifling error would have been obviated. The scale in the drawing marks eighths of inches, and my instrument only recorded quarters. Doubtless Van Huevel's instrument, which is more widely known, is capable of equal accuracy. I have had much satisfaction in its use in measuring this diameter of the brim, and it is the only one I have ever used which is capable of measuring all the diameters of the pelvis. When the true conjugate is not obstructed, the instrument devised by my friend Dr. Samuel W. Francis has given me great satisfaction.

In using Lumley Earle's pelvimeter in the conjugate diameter, it will probably be found most efficacious when the patient is lying on her side, with the concavity of its curve looking towards the concavity of the woman's sacrum—a method exactly the reverse of that understood by me to be recommended by the inventor.

I have found it necessary that an assistant should read off the mark on the scale, while I was satisfied that the points were properly placed, and have repeated the manœuvre several times before deciding the matter.

The instrument has not seemed to me of value in the transverse and oblique diameters. In the former, the instrument devised by Dr. King is the best with which I am familiar; but I do not think that I shall try it any more in measuring the conjugate, as I am satisfied that it is badly adapted to that measurement, and inferior to other pelvimeters. In the use of of Van Huevel's instrument a skilful assistant is also very desirable.

In conclusion, I desire to express my conviction that the efforts of so many men to devise an instrument on which the profession may unite, are efforts in the right direction; and that the profession will gain in accurate study of the pelvis by the use of those already invented. I regret that in the past I have not in many cases carefully studied all the diameters of the pelvis instead of measuring one alone; and I hope in the future to diminish sources of error, and clear up indications for treatment, by systematic study of all.

3. *What indications for treatment do they present?*

Since, then, we can measure the contracted dimensions of the pelvis with sufficient accuracy, can we readily decide the proper time for the induction of labor in a given case? We find in text-books, and in monographs, tables which give the approximative diameters of the fœtal head at various periods of its development, and differences of a quarter of an inch are spoken of with great positiveness. I guide myself by these tables, but with a wide margin of allowance; and greatly aid my diagnosis of the size of the fœtus by careful abdominal manipulations, and measurement of the head—if it present—by conjoined manipulation, with the fingers of one hand in the vagina, and those of the other pressing upon the head above the pelvis. In this way we can form some idea of the size of the head in a given case, before labor sets in, and of its relations to the maternal pelvis. In the five cases in which I have induced premature labor for deformity of the pelvis, the head of the viable fœtus has always been able to pass the contracted diameters without any more difficulty than I expected or was prepared for. Still this element of uncertainty weighs heavily on the mind that longs for a favorable result to both of the subjects of the operation. When I look over my memoranda of cases of contracted pelvis, only seen when labor had commenced, and read the record of the efforts made by myself and others to deliver, and note in some of them the failure of forceps, the failure of version, and the final resort to the perforator, I cannot help fearing that deferring the induction of labor too long for the sake of the child, may yet subsequently involve these same serious risks.

It does not come within the province of this paper to discuss the indications offered for the different obstetric operations by degrees of pelvic deformity; but I may refer again to the case in which I delivered a living child with forceps through a three-inch conjugate as showing that such an operation is possible; nor does the death of the child on the second day from an apoplectic effusion detract from the proven availability of a properly constructed light forceps, with narrow blades, in such a contingency.

The advantages of conjoined or bi-manual manipulation of the head in these cases, as well as the ease with which we can sometimes remove threatening risks by timely treatment, are illustrated by a case in my wards in Bellevue. The patient was a primipara; and when I saw her, the tension of the membranes prevented the presenting part from being reached. One fœtal heart could be heard; but I gave my opinion, from inspection of the abdomen and the existence of a sulcus to the right of the median line, that there were twins superimposed. The sacral promontory could be felt to project, and I estimated the conjugate with the finger to be three and a half inches. The membranes then ruptured spontaneously, when two heads could be felt presenting—one to the left, and superiorly, with its membranes still unruptured; the other (of which the membranes had ruptured) to the right and beneath. The lower seemed likely to advance first and catch beneath the upper. Fearing the risk of locking, I ruptured the membranes of the upper child, placed my hand on the abdominal wall over the head, which was superior and to the left, and forced it into the pelvis in advance of the other head. This one was subsequently delivered first by powerful uterine contractions, and fifteen minutes afterwards the second child followed. First child, female, weighed 4 lbs. 4 oz.; second, male, 4 lbs. 8 oz. Both living. Mother did well.

Now, had I seen this patient before labor set in, and recognised the deformity, I might have been greatly embarrassed by the difficulties in the way of reaching the presenting part of the child. It is probable that, as I diagnosticated twins before the waters broke, I might have made that diagnosis before labor set in. In these and similar contingencies,

I consider that premature labor would have been indicated; nor am I sure that when the small size of the children is noted, and the liability to premature labor in twin cases remembered, the case may not, after all, have been one of spontaneous premature labor. It has occurred to me that there might be some embarrassment in such a procedure as was practised here, in countries where the rights of primogeniture might have thus been summarily decided by the accoucheur in favor of a particular twin, had both been boys. Still, in this procedure, the twin excluded from the birthright could not even claim, on such evidence as that of the scarlet thread in the case of Tamar, the daughter-in-law of Judah.

It is my conviction that the difficulties attending the recognition of the dimensions of the pelvis, and the size of the unborn child, should lead to a more liberal resort to the induction of premature labor than is generally practised in these cases. To insure this, will demand, in the first place, that the profession should regard the procedure with even more confidence in its simplicity and success than is yet generally accepted; and, in the second, that women should be more willing to submit thereto. It is well known that in every country the greatest difficulty is very often experienced in convincing women of the desirability of the operation, even when their lives have been more than once perilled by the severest operations for the extraction of the fœtus.

It is evident that the public mind can only be reached through the profession, and that much remains yet to be accomplished with both.

Most practitioners would fear that if they recommended and induced premature labor in one pregnancy, and it were not shown to be necessary in subsequent confinements, blame might attach to them. This is a very narrow view to take of the subject. Blame will only be deserved if such practice became the rule, and not the exception. Moderate degrees of deformity, and even serious degrees of deformity, present great differences in the results of labor allowed to come on spontaneously. Without claiming that labor may be delayed beyond the customary period of gestation, it is unquestionable that in very many cases it occurs a few weeks before, without

attracting much notice even from the patient. In very many cases the period of fecundation is not known, or may be assigned to a wrong date. Such conditions modify a labor. The difference in the size of a child in different pregnancies is not only felt in the range from four pounds to twelve, and in the probability of increased size in ratio to the number of pregnancy, but a material influence is exerted by the different size of the head in two children weighing alike, as well as in fortunate adaptations to special requirements. Thorough flexion in one case as compared with imperfect flexion in another; accurate dip of the head in one case instead of an oblique direction; fortunate correspondence of the head to the diameter, which may be roomier than the other; varying expulsive forces; differences in the ossification and capabilities for moulding of the head, with many other influences, affect the results of different labors. Some of these we can control in a given case, and some we cannot; while most may be resolved by inducing the labor.

These considerations, therefore, joined to those which lead me to think that premature labor may be induced with great safety to the mother, make me an advocate for a more extended resort to the procedure in cases of pelvic deformity, than would be conceded by many. Some of the arguments against a more extended use of this procedure have already been considered; others will be based on the increased liability to abuse which such practice would involve. While all will admit how illogical it is to argue from the abuse against the use, we are compelled, alas! to recognise serious weight in the objection. But, after all, are competent men, desirous of diminishing serious risks to two lives entrusted to their care, to be withheld from practising what they believe to be best in a particular case, by the fear that it may prove a precedent for evil?

The fifth case reported in this series comes partly within this category. In the first labor, very competent men had failed with the forceps, and the perforator had been most reluctantly resorted to, while great difficulty had occurred in the delivery of a mutilated child through the contracted brim. Under such circumstances, the induction of premature labor in the next confinement would, of course, be admitted as jus-

tifiable under a general law. But yet I found no great deformity. An antero-posterior and a transverse diameter of four inches, some diminution of the oblique diameter terminating in the right acetabulum, and no other deformity. This woman may hereafter have living children, at term, without assistance.

Perhaps even this theory, which has occurred to me, may suggest one impediment in the first labor. After that delivery, she had great hardness, swelling, and then fluctuation in the right iliac region. An opening was made there, and a counter-opening became necessary in the vagina. A free discharge took place, some of which resembled honey; and finally, under iodine injections, the sinus closed. Now it is possible that a small ovarian, or parovarian tumor, may have accidentally diminished the contracted transverse diameter. May not this have become inflamed, and may not its contents have escaped with the results of peri-ovaritis? At the time when I examined the case, no trace remained of all these serious post-partum conditions but the large scar in the abdomen. Now, while, of course, the history of this previous inflammation lent additional force to the arguments for inducing premature labor, the great question turns, after all, on the accurate measurement of the pelvic diameters; and the decision first to postpone the operation until a late period of pregnancy, and, secondly, of inducing it at all, was based on its result.

I claim that, in such cases, it is better to induce labor at a late period of pregnancy, or at least by pelvic measurement and careful examination of the probable size of the child in that particular pregnancy, to be prepared in every manner with data for the mature decision of a competent consultation. We may carefully base general laws on vast accumulations of statistics, but the liabilities to error involved in these large tables will never relieve us from anxious study regarding the phenomena of an individual case.

The question of procuring abortion, in cases where the passage of a viable child is rendered impossible by pelvic deformity, is one of the most painful and difficult cases that is offered for decision in the whole range of medicine, and one which, I am glad to say, has never been pressed on me. We too often find a reckless disregard for fœtal life based on the

paramount claims of the mother for consideration on the one hand; and we may find consideration for the life of the child carried to the extreme of bigotry on the other.

I am an advocate for a more frequent, and beyond all, for a more timely resort to the Cæsarean section, under favorable circumstances, than has been the rule hitherto. But the prospect for these favorable circumstances demands careful study. In a woman condemned by social relations to dwell in Paris, for instance, where I believe that no success has followed the operation for more than a century, my voice would be for abortion. If the decision were based on this consideration alone, it ought to fall before the ability of the patient to remove into a locality better adapted by climate for success. This consideration should always enter into the treatment of every case. I would prefer to perform Cæsarean section elsewhere than in a crowded hospital, and especially so in the winter months. Perhaps the favorable results now attending ovariotomy in Strasbourg and other parts of France, presage the increased chances for success which will hereafter attend Cæsarean section under like circumstances. An unexpected recognition of this deformity in a woman pregnant for the first time with a non-viable child, offers a test question for the rigorous adhesion to our duty towards the living immature fœtus, or for the admission of more sympathetic considerations for the most unfortunate mother. Under just such circumstances as these, the mother's appeal would find an answering echo in my heart, if she urged that appeal after full appreciation of her situation in all its relations. If she became pregnant again, the case would be different. We cannot release an intelligent being from dangers knowingly incurred, unless our refusal is based on other considerations than those of pelvic measurements alone. The arguments of Radford appeal to me irresistibly. There is a difference between a just sympathy and a sickly sentimentality.

But how important here are trustworthy measurements. It is no longer a question between the delivery of a viable child by art through the pelvis, and the ability of nature to complete the process at term. In such a case the error is venial; in the other it partakes indubitably of the nature

of a crime. Doubt should lean towards the Cæsarean section.

The argument in favor of procuring abortion when a woman is forced to become pregnant again by a brutal husband, will be decided differently by different men. It strikes me, however, that this cannot be a plea in equity, at least; for the law should protect, by separation, a woman proven to be unfit for the consummation of the marital act by such perilous pelvic deformity.

The desirability of accurate pelvic measurement; of the induction of premature labor, or of abortion, in cases of pelvic deformity where the Cæsarean section might be deemed unjustifiable; and of the Cæsarean section in a case where craniotomy would of necessity offer an amount of risk not less than that of the Cæsarean section, as well as the reluctance of deformed women to take advantage of the induction of labor—has been deeply impressed on my mind by the difficulties that are recorded in the practice of the most expert obstetricians, as well as by the cases that have come under my personal observation. But no one has ever proved more impressive and illustrative than one which occurred during the period of my residence, as a pupil, in the Dublin Lying-in Hospital, in 1849, and which has been reported by Dr. Shekleton in the *Dublin Quarterly* for Nov. 1850, and by Drs. Johnston and Sinclair in their admirable work.

Anne Parsons entered the hospital in labor for the sixth time, on the 20th July, 1849. Her first child was dead-born; her second delivered by the crotchet; her third, acephalous; her fourth and fifth, by the crotchet. The last delivery had taken place three years and a half before. In this fifth confinement it required two hours of active and unremitting labor of three gentlemen to drag the crushed head through the pelvis, which was deformed by an immovable pyriform tumor, fibrous in character, extending from near the coccyx to the superior brim of the false pelvis. The true pelvis was nearly filled, with the exception of a small passage on the right side. With this history, Anne Parsons entered the hospital in labor, with the fœtal heart beating. It is obvious that in this city such a case would be deemed a proper one for the Cæsarean

section. But we know the tendencies of the Dublin school towards craniotomy; and I remember well, that Prof. Montgomery then told me that there had been no case of Cæsarean section successful in Ireland but one, performed by a drunken midwife at a fair.

There were present at this consultation, Dr. Shekleton, the master of the hospital; Drs. Geo. Johnston and Sibthorp, assistants; Drs. Collins, Evory Kennedy, and Johnson, formerly masters of the hospital; Drs. McClintock, Hardy, and Denham, formerly resident assistants; Sir Philip Crampton, consulting surgeon to the hospital; Harrison, the eloquent and widely known professor of anatomy; Cusack, surgeon to the Steevens Hospital, a man of great practical experience and marked sagacity; Hamilton, surgeon to the Richmond Hospital, well known by his writings, and well remembered for his unostentatious hospitality in that most hospitable of cities. It is difficult to group together men of greater experience in practical midwifery, and of more brilliant and varied attainments. The presence of Churchill, Montgomery, and Beatty alone was wanting to render it complete.

The master of the hospital, Dr. Shekleton, an ex-army surgeon who wore his clasps for the whole Peninsular campaign, and one of the best obstetric operators that I have met, prepared a small room for the Cæsarean section, and unhesitatingly avowed his preference for the operation. In the consultation which followed, craniotomy was decided upon, on account of its possibility, and on account of the success which had attended the previous performance of the operation in the last confinement. The responsibility for this decision rested mainly on the opinion given by Drs. Johnson, McClintock, and Denham, that the tumor had not increased in size during the three years and a half that had elapsed since the previous confinement. The majority of the obstetricians hence decided for the same operation, and the rest deferred. Sir Philip Crampton, whose exalted position and singular charm of manner lent such weight to his opinion, declined to enter on a question so purely obstetric.

The head was perforated, and left to be moulded by the uterine contractions; but during the interval the presentation

changed, and the left arm slipped into the pelvis. Evisceration was then commenced, and the decapitated head withdrawn last of all, after three hours of continuous efforts by Dr. Shekleton and others. Rupture of the uterus took place twenty minutes before the operation was concluded, and the woman died ten minutes after the head was extracted.

Such cases illustrate a part of the difficulties that attend the practice of obstetrics. Do what we may, we can never prevent the final tribute which death claims from all; but by the exercise of our art we can diminish the rates of mortality in populations, and from special risks. That the timely and skilful use of the methods to which we have this evening directed our attention will prove most efficacious in diminishing the dangers of labor to mother and child, must be admitted; and we must also admit that these methods are not, even yet, widely enough known and sufficiently employed.

My purpose will have been fully accomplished, if this paper can draw forth the unrecorded experience of the Fellows of this Academy, and can even remotely contribute to the welfare of mankind. While thanking the Academy for the attention which so long a paper is so well adapted to weary, I may venture the prophecy, based on my convictions of the simplicity of these methods in competent hands, and on the certainty of their future improvement, that the day may come when it may not be considered an objectionable precedure to assign a day for bringing on the labor at the close of pregnancy in favorable cases, even for considerations of personal convenience to the pregnant woman, or to admit of her receiving, under peculiar circumstances, the attentions of a selected physician.

A MEMOIR

ON OSTEO-MYELITIS.

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Mr. President and Gentlemen of the Academy.—The term osteo-myelitis, derived from two Greek words, οστεον, bone, and μυελος, medulla or marrow, signifies *inflammation of the medullary tissue of bone.* As the word periostitis is employed for a symbol to represent inflammation of the tough periosteal membrane, and ostitis to represent inflammation of the calcified bone-tissue, so the term osteo-myelitis is used to signify inflammation of the soft medullary tissue, or marrow. The statement, therefore, of M. Jules Roux, representing that osteo-myelitis is inflammation of bone in its totality, is not correct. For the production of the last-named affection, it is requisite that there should be not only inflammation of the medullary tissue, but also inflammation of the osseous tissue, or ostitis, and inflammation of the periosteal membrane, or periostitis. Now, while it happens not unfrequently that inflammation of the marrow is associated with either coincident or consecutive inflammation of the bone-tissue and the periosteum, and this remark is especially true of cases of this disease that have either become chronic or have a spontaneous origin, it also happens not unfrequently that we meet with cases wherein the inflammation of the medullary tissue has reached the stage

of suppuration, and at the same time the osseous tissue and the periosteum present a natural appearance, a circumstance which we have sometimes witnessed in the stumps of amputated limbs that have been fatally involved in the acute forms of the disease. It is clear, then, that osteo-myelitis may exist as an independent affection, at least so far as inflammation of the adjoining tissues is concerned; and it is in that sense that we shall employ the term.

During the late internecine strife in this country, this disease proved very destructive to our wounded soldiers, and was one of the chief causes of a fatal result in cases where the osseous tissue had been injured by gunshot projectiles, or surgical operations involving bone had been performed. It was by this circumstance that my attention was first called to, and then fixed upon this disease. It was by this circumstance that I have been induced to devote some time to the study of its nature and relations.

On investigating the literature of the subject, I find that the disease now known by the name of osteo-myelitis is not a new thing. Cases and fragmentary accounts of said disease, under a considerable variety of names and appellations, have, from time to time, been placed on record by a considerable number of surgeons, commencing more than one hundred and fifty years ago. *J. L. Petit* speaks of it in his Treatise on the Diseases of the Bones (anno 1705), in the chapter on exostosis, and presents it as one of the effects of those osseous tumors which, instead of growing out from the surface of the bone, are developed towards the medullary canal. *Duverney*, in a chapter where he treats of that form of fracture of the long bones which is sometimes called a fissure or splint, relates three cases of it, one of which, especially, leaves no room for doubt with regard to its real character (Nélaton). In 1740, *Gooch* saw a patient having osteo-myelitis, which he has quaintly described as "A case in which the tibia was affected to an extraordinary degree in a very short time, by a critical discharge of febrile matter upon a leg, which had been fractured some years before." (*Gooch's Surgery, Vol. II., p.* 349.) *Cheselden* says: "Sometimes matter is formed in the large medullary cavities of the cylindrical bones, which, constantly

increasing and wanting vent, partly by corroding and rendering the bone carious, and partly by pressure, tears asunder the strongest bone in an human body, of which I have seen several instances." (*The Anatomy of the Human Body, by W. Cheselden, p.* 40. *London*, 1778.) He refers to two cases which he had seen, but does not give the particulars. The celebrated English surgeon, *Hey*, reports two cases of suppurative osteo-myelitis, circumscribed in character, at considerable length, under the title of "Caries of the Tibia." An abscess formed in the medullary canal in both of them, which discharged its contents through a perforation or hole in the walls of the tibia made by nature, after the method called by some *spontaneous trepannation.* The first case occurred in 1786, in the person of a young lady from Richmond. The second in 1792, in a stout young woman, aged fifteen. *Hey* did not amputate, but enlarged the opening in the walls of the bone already made by nature, and removed the diseased tissues lying within the bone. Both patients made good recoveries. (*Hey's Surgical Observations, pp.* 26 *to* 32. *London*, 1810.) *Percival Pott*, in a chapter devoted to "Separation or Destruction of both Tables of the Skull from Contusion," relates at least two cases of osteo-myelitis, involving the diploë; and, while describing the inflammatory consequences of contusion of the cranium, he gives an excellent account of the symptoms produced by cranial osteo-myelitis. (*Pott's Chirurgical Works, Vol. I., p.* 115, *et seq. London*, 1779.) *Abernethy* says: "Suppuration of the diploë, and the death of a portion of the bone, are the common effects of injury done to the cranium; and such a morbid state may indeed occur at some distance of time from the receipt of the injury." (*Surgical Observations. Vol. II., p.* 69. *American ed.*, 1811.) He here obviously speaks of cranial osteo-myelitis. *Hennen*, writing in 1817, alludes to this disease as it affects the long bones, under the name of abscess in the medullary canal, etc. (*Principles of Military Surgery, pp.* 100–115. *Am. ed.*) In 1828, *Craigie* made the following statement in his book on pathological anatomy: "The medullary filamentous web is perhaps still more important than the periosteum, in its morbid influence on bone. It is, in the first place, liable to inflammation; and, accordingly as this

takes place in the medullary web of the cylindrical bones, or in that of their epiphyses, or of the short irregular bones, different effects result." (*Op. cit.*, p. 564.) In 1831, *M. Reynaud*, a French surgeon, published a paper "On Inflammation of the Medullary Tissue of the Long Bones," in which he gives an account of five cases of that disease, all occurring in stumps after amputation performed for diseased conditions, and all terminating fatally. (*Archives Générales de Médecine, tome XXVI., p.* 161 *et seq.*) In 1833, *Mr. B. Phillips*, of Westminster Hospital, wrote a brief but excellent article "On Inflammation of the Medullary Membrane after Amputation." (*Lon. Medical Gazette, Vol. XIII., p.* 189. *Nov.* 9, 1833.) In 1833, also, *Dr. Carswell* gave a picture representing inflammation of the medullary canal of the femur. (*Illustrations of the Elementary Forms of Disease, 4to.*) *Cruveilhier* evidently alludes to cases of osteo-myelitis, although he does not use that term, while discussing the subject of "Phlebitis and Visceral Abscesses," in his "Pathological Anatomy of the Human Body." (*Tome I. Liv. XI., p.* 10.) In 1844, *Nélaton* gave a brief account of that disease, following closely the paper of Reynaud. He also employs the term osteo-myelitis. (*Elemens de Pathologie Chirurgicale, t. I., p.* 595 *et seq.*) Jules Roux says this term was devised by Nélaton in 1834. It has therefore been a long time in use (thirty-two years), although it is the recent designation of the disease. (*Mémoires de l'Academie Impériale de Médecine, t. XXIV., p.* 553.) In 1849, *Mr. Stanley* wrote about it in a very interesting way, and narrated seven cases under the title of "Suppuration in Bone." (Vide *A Treatise on Diseases of the Bones, etc., etc., p.* 48 *et seq. Am. ed.*) In 1855, a valuable memoir on osteo-myelitis, written by *Dr. T. Vallette*, a French military surgeon, was published in the "Recueil de Mémoires de Méd. et de Chirurg. Militaires," a publication which annually emanates from the French minister of war. *M. Vallette* describes the disease as he saw it in the Crimean war, at one of the large military hospitals of Constantinople, among the wounded brought directly from the battles of Alma and Inkerman. The disease appeared in an acute form, and proved very destructive to life. In a work "On Suppuration and Surgical Drainage," published in 1859, *M. Chassaignac* devotes thirty-one pages to an

excellent discussion of osteo-myelitis, as it occurs spontaneously in civil life and unconnected with wounds or surgical operations involving bone, in which he gives a full account of four cases observed by himself. In 1860, a long and very interesting memoir upon traumatic osteo-myelitis, written by *Dr. Jules Roux*, and presented to the Imperial Academy of Medicine, at Paris, was published in the transactions of that body. (Vide *Mémoires, etc., p.* 537 *et seq.*) *M. Roux* had charge in 1859 of the Naval Hospital of St. Mandrier at Toulon, into which he received about two thousand wounded from the battle-fields of the Italian campaign of that year. He saw and described the disease under its chronic conditions, that is, after it had existed for a considerable time. In 1860, after the presentation of M. Roux's memoir to the Academy, osteo-myelitis was the subject of discussion at two successive sittings of that body. It was also the theme of an elaborate discourse by *Baron H. Larrey*, the distinguished Surgeon-in-Chief of the French Army in the Italian campaign, and the subject of a letter to the Academy from *M. Legouest*, the distinguished surgeon of Val-de-Grace. In short, M. Roux's memoir appeats to have produced a very deep impression upon that assemblage of learned men. *Mr. Longmore* read a valuable paper on osteo-myelitis before the Royal Medical and Chirurgical Society, February 28, 1865, an abstract of which was published in the Medical Times and Gazette, March 11, 1865. (Vide *American Journal Medical Sciences, July*, 1865, *p.* 230 *et seq.*) But the various systematic treatises on surgery in use in both Europe and this country contain either no account at all, or only a very meagre one of this important disease.

In reviewing the literature of the subject, we are struck by the fact, that it is only within the last dozen years, that osteo-myelitis has fairly begun to attract the attention which its importance demands. The cause of surgical science owes much to Doctors T. Vallette and Jules Roux, for bringing prominently forward their own observations concerning this very destructive disease, as it appeared in the Crimean and Italian campaigns respectively. The experience of those two wars, together with that of our own fratricidal strife, have shown conclusively that osteo-myelitis occurs much more frequently

in military practice than has hitherto been generally supposed; and I believe that testimony is now accumulating which tends to prove that this disease occurs in civil practice also much oftener than has heretofore been imagined. Its existence explains the want of success which oftentimes attends surgical operations involving bone—operations undertaken for the removal of disease as well as for the relief of injury, and occurring in civil as well as in military practice.

Indeed, the physiological and pathological importance of the medullary tissue has, for the most part, been overlooked until a very recent period. It is only of late years, and since the aid of the microscope has been secured in prosecuting the study of histology, both healthy and morbid, that the importance of the medullary tissue to the well-being of the bones has been sufficiently recognised. Formerly, the marrow in the canals of the long bones appears to have been looked upon as a tissue analogous in every respect to the tela adiposa, and as a matter of quite secondary importance, so far as the pathological processes in bone are concerned.

I have thus sketched the historical features of our theme at considerable length, because, by so doing, we get broader views of the subject, and are enabled to perceive the development of ideas, and the progress of knowledge concerning it. In 1831 the memoir of M. Reynaud appears to have fallen coldly upon the medical mind of France. But in 1860 the memoir of M. Jules Roux, on the same subject, excited a very lively interest in even the staid Academy of Medicine at Paris. Thus we behold progress in the midst of even the conservatism of traditional medicine. I have done this also for the purpose of showing that inflammation of the medullary tissue is not a new-fangled notion, but a disease which, under various appellations, has been known for a long time; and that it is not an affection of rare occurrence, but one which is frequently met with, and as frequently recognised, if the examination, whether clinical or post-mortem, be sufficiently thorough and complete.

Since the anatomical distribution of the medullary tissue is such that it may be found in greater or less quantity in every bone* of the body, it follows that osteo-myelitis may be pro-

* *Robin* says: "Marrow is found in all the bones of the body, and it extends

duced in any bone of the body. It has, indeed, been met with in several of the cranial bones, in the upper jaw, in the lower jaw (Prof. Clark, a distinguished fellow of this Academy, has seen two examples wherein this bone was affected), in the sternum, ribs, humerus, radius, ulna, carpus, os innominatum, sacrum, femur, tibia, fibula, patella, and the bones of the tarsus, by various observers. The disease, however, occurs much more frequently in the lower than in the upper extremity, obeying in this respect the general law concerning the development of disease in different parts of the body.

Varieties of Osteo-Myelitis.—The clinical history of this disease exhibits a wide diversity with respect to intensity and duration. In some cases its severity is very great, and it runs through its whole course in a few days; while, in other cases, its progress is sluggish and obscure, requiring, not unfrequently, several weeks, or even months, to attain a full development. Inflammation of the medullary tissue of bone may, on the one hand, be ranked among the most rapidly destructive of all inflammatory disorders with which we are acquainted; while, on the other hand, it appears to be allied to those that are exceedingly slow in their march. There are but few parenchymatous inflammations which destroy life more speedily than acute suppurative osteo-myelitis, and there are none which are slower in their progress than chronic abscess of bone. For the purposes of clinical study and description, and likewise for the development of certain practical considerations connected with the treatment, it will be found convenient to recognise at least *two varieties of osteo-myelitis*, namely, the *acute* and the *chronic*. But, at the same time, it should not be forgotten that this classification, although it is founded upon differences which actually exist in nature, is, in reality, artificial; that cases of this disease, in respect to two of their elements intensity and duration—form a graduated series, represented at

through the vascular canals as far as the periosteum." Indeed, the medullary tissue is not only distributed widely, but is also abundant in quantity. It fills the canals of the long bones, the cancelli of their epiphyses, and of the short and irregularly shaped bones in general, the diploë of the cranium, and the medullary spaces in every part of the osseous system.

one end of the scale by the most acute, and at the other by the most chronic cases; that in the middle of this descending series it is difficult to say whether some of the cases belong to the acute or to the chronic class; and that the inflammatory process is essentially the same in both varieties, so far as the marrow is concerned.

The following case presents a good example of acute inflammation of the medullary tissue, following amputation, as it appeared in our army hospitals during the late war.

A Case of Acute Osteo-Myelitis.

Private Henry Stubblebine, Company C, Ninety-sixth Pennsylvania Volunteers, aged twenty, and of sound constitution, was brought to Stanton U. S. Army General Hospital, May 8, 1863. He had been wounded in the battle near Fredericksburg, Va., five days before (May 3d, 1863), and on that account had suffered primary amputation of the left thigh at the lower third, by the circular method.

When he came to the hospital his general condition was good; a water-dressing was applied to the stump, and his bowels being constipated, a dose of ol. ricini was administered.

May 11.—Oozing of blood from the stump was observed. The stump also had appeared to be swelled (distended) ever since he was admitted to the hospital. It was opened, and found filled with coagulated blood, which was removed. It was left open to heal from the bottom by granulation, and to prevent any possible accumulation of pus in its interior.

May 15.—Suppuration free, and laudable in quality; granulations healthy; the stump is also contracting. He continued to do well till May 27. Has passed a restless night, and complains of a good deal of dull pain in the stump, which he refers to the bone; has no febrile symptoms; ordered morph. sulph., in full doses.

May 28.—The pain continues, but is more severe; pulse quick, skin hot and dry; pus oozing from the sawn end of the bone in the stump (osteo-myelitis); stump swelled, secretion of pus diminished; prescribed potass. iodid. ℈j., dissolved in aqua camphor. f. ℥ iv., to take a tablespoonful every four hours, and stimulants. In the afternoon he had a chill.

May 29.—Was slightly delirious; had rigors and hot flushings at irregular intervals; was rapidly becoming debilitated; same treatment continued.

May 31.—Delirium increased; skin yellow, pulse 130, and very feeble; continued in about the same condition till June 2, when he died.

The stump was examined after death, and a number of small abscesses were found in the muscular tissue of it. Pus was also found in the medullary canal of the femur, forming numerous little collections, varying in size from a mustard-seed to a pea. The internal organs were not examined, as his friends were waiting to remove his body from the hospital.

Comments.—This is a strongly marked instance of acute osteo-myelitis. The following are the essential points pertaining to it: A healthy young man of twenty years sustained primary amputation of his left thigh, for gunshot injuries. He seemed to do well for a period of twenty-four days. Then symptoms of osteo-myelitis appeared in the stump-bone. Of these symptoms, the first was a dull, aching pain in the stump, which the patient referred to the bone. This pain was severe enough to deprive him of sleep. The next day the pain was still more intense, and he now had constitutional disturbance in the shape of irritative fever. At the same time the stump became swelled, and the flow of pus from it diminished in quantity and depraved in quality, showing that the soft tissues of the stump sympathized with the inflammatory mischief which had been lighted up in the medullary tissue of the bone, or, speaking more exactly, that the inflammatory process had spread from the marrow to the soft parts lying exterior to the bone. Pus was also seen oozing out from the marrow at the end of the stump-bone. This occurrence established the diagnosis of osteo-myelitis beyond a doubt. On the next day (the third after the appearance of inflammation of the medullary tissue), his symptoms were all worse. He was beginning to be delirious, and his strength was failing rapidly. He also had rigors, alternating with hot flushings and occurring at irregular intervals. The last group of symptoms belongs to pyæmia. On the next day (the fourth) he was still more delirious, and still more debilitated. His skin had now assumed a

yellowish hue, and the symptoms of pyæmia were still more pronounced. He continued to grow worse, and died on the seventh day after the symptoms of osteo-myelitis made their appearance. The inflammation of the marrow was suppurative in character, and it is worthy of special notice that in the short period of three days it was attended by signs of general purulent infection. It is probable that an examination of the internal organs would have revealed visceral abscesses. The succession of pathological events in this case is clearly defined. The patient did well for more than three weeks; then his stump was attacked with suppurative osteo-myelitis, and pus was seen oozing from the marrow at the end of the bone. In a few days symptoms of purulent infection made their appearance, and a few days later still he died of pyæmia. The focus of suppuration from which the system became infected in this case was obviously located in the medullary tissue of the stump-bone.

The cases of osteo-myelitis reported by *Hey*, under the name of "caries of the tibia," to which reference has already been made, present examples of the chronic variety of the disease.

Cases of chronic osteo-myelitis were very often met with in our military hospitals during the late war. The disease manifested itself in this form mainly in the stumps of amputated limbs, and in gunshot fractures which were being treated conservatively. Chronic inflammation of the medullary tissue of stump-bones was generally accompanied by necrosis and exfoliation of the dead bone. At the same time, the portion of bone that did not perish became more or less thickened, in most instances, by the formation of laminæ of new osseous tissue around the exterior (periostosis). The army medical museum at Washington contains a great number of specimens of necrosed bone that have been extracted from stumps in cases of chronic osteo-myelitis. Such patients generally made good recoveries.

Causes of Osteo-Myelitis.—The time at our disposal will permit us to state in this place, concerning this topic, but little besides the conclusions to which we have been led by our investigations.

1st. Osteo-myelitis is not unfrequently produced by *contusion*

and contused wounds of bone. Pott and Abernethy have abundantly shown that contusion of the cranium often occasions suppuration in the diploë. The writer published in the American Journal of Medical Sciences for July, 1865, several cases of contused wounds of the long bones, inflicted by gunshot projectiles, wherein destructive inflammation of the medullary tissue had been produced. Indeed, the chief source of the great danger to life which attends contusion and contused wounds of bone, arises from the inflammation of the medullary tissue, which is very apt to be kindled thereby.

2d. Osteo-myelitis is not unfrequently occasioned by *compound fracture*, especially gunshot fracture of the long bones. In such cases, the inflammation of the marrow appears to be due not only to the injury it has sustained, but to the admission of air, also.

3d. This disease has been produced by the operation of *resection*, or the excision of a portion of a bone. Of this I have seen several instances.

4th. Osteo-myelitis often follows the performance of *amputation*, especially the major amputations, in both civil and military practice. This has been witnessed by Reynaud and Phillips, by Vallette, Pirogoff, and Roux, and by our own army surgeons during the late war. The first two made their observations exclusively in civil practice.

5th. *Foreign bodies*, such as a bullet, a piece of clothing, or a fragment of bone, when driven into and lodged in the medullary canal of a long bone, have been known to produce fatal osteo-myelitis. Of this I saw an instance not long since in civil practice. The foreign body was a fragment of bone, and the original injury, gunshot fracture of the femur.

6th. The *scrofulous dyscrasia* may induce inflammation of the medullary tissue. Stanley relates two cases belonging to this category. At least one of Chassaignac's cases also was probably occasioned by struma.

7th. *Constitutional syphilis* sometimes occasions osteo-myelitis. *Percival Pott*, admitted by all to have been a very acute observer, states that in cases of syphilitic necrosis of the cranial bones, the disease has its seat in the diploë, or in other words, in the medullary tissue. *Mr. B. Phillips* and *Langston Par-*

ker have reported cases of syphilitic osteo-myelitis occurring in the long bones of the lower extremity, especially the tibia.

8th. The *rheumatic diathesis* sometimes appears to be concerned in the production of osteo-myelitis; or, in other words, we have not only a rheumatic periostitis and a rheumatic ostitis, but also a rheumatic osteo-myelitis. In one of Stanley's and one of Chassaignac's cases the disease seems to have had a rheumatic origin. Dr. Hermann Klose, of Breslau, has given an account of thirteen cases of what appears, in reality, to have been osteo-myelitis under the unique title of *Meningo Osteo-phlebitis* (Vide *Archives Générales de Médecine* for August and November, 1858.) Dr. Klose distinctly recognises the idea that the disease may have a rheumatic origin.

9th. *Idiopathic fever* is occasionally followed by osteo-myelitis. Indeed, this occurrence took place in one of Hey's cases, and he makes the following remarks concerning it: "Upon a review of the case, I am inclined to think that an abscess was formed within the tibia, in consequence of the fever which she had in May, 1786. During the continuance of the fever, she had no particular pain in her leg; but upon the decline of the fever the pain commenced, and continued violent for six weeks. It seems probable that during this time the matter was making its way through the anterior lamella of the tibia, and that the pain abated soon after the matter had perforated the bone; for it ceased immediately on the appearance of a tumor on the shin." (*Op. cit., p.* 26 *et seq.*)

10. Osteo-myelitis sometimes occurs *without apparent cause*, of which Stanley has related two instances.

11th. The *transportation of the wounded*, in military practice, is often attended with unavoidable injury of such a nature as to assist not a little in the production of osteo-myelitis, among such of the wounded as have sustained an injury of the osseous tissue. For example, in cases of gunshot fractures of the long bones of the lower extremity, the jolting incident to ambulance transportation causes the soft parts to be pierced and lacerated by the sharp ends of the broken bone, and sometimes it happens that a fragment of comminution is driven into the medullary canal. Again, in cases where the thigh has been amputated, the jolting causes the soft parts of the stump, but more

especially the flaps, to be bruised against the bone, in consequence of which they become more or less inflamed. Now, this traumatic inflammation may spread to the medullary tissue and kindle in it a suppurative inflammation of a fatal character. However, it seems to be well established, whatever the explanation may be, that, other things being equal, the wounded who are treated on or near battle-fields suffer a much smaller loss from osteo-myelitis than those who are transported to distant parts for surgical care. The writer's personal observations on this point are corroborated by the experience of Surgeon Henry Janes, U. S. Volunteers, at Gettysburg, and likewise of the Confederate Surgeon S. D. Moses, at Charlotteville, Va.

12th. This disease is much aggravated by *impure air, especially the foul air of imperfectly ventilated or uncleanly hospitals.* This is probably one of the most prolific of all the causes of osteo-myelitis; and since it is in great measure a preventible cause, it should claim our earnest attention. Observation has shown that, other things being equal, the wards of a hospital that are most impure in respect to their atmospheric condition, generally furnish the largest proportion of fatal cases of osteo-myelitis. Observation has also shown that surgical patients—for example, those who have sustained gunshot fracture, or amputation, and are treated in the portions of a ward where the air is most likely to stagnate, for example the corners, are considerably more liable to become affected with destructive osteo-myelitis than those who are treated in other portions of the same ward where the air is more free from impurities. Observation has further shown that surgical patients treated in hospital tents, if they are properly pitched and policed, are much less likely to be seized with fatal osteo-myelitis than a similar class of patients treated in the wards of a hospital building. It is probable that under the depressing and poisonous influence of foul hospital air, cases of inflammation of the medullary tissue which, under favorable circumstances, would eventuate in recovery, degenerate into the stage of suppuration, and perhaps a foul suppuration at that, and sooner or later purulent infection is produced, which occasions death.

Osteo-myelitis is also pre-eminently a disease of *youth.* Of seventy-two cases, the notes of which have been preserved,

that occurred under my observation in military practice, the ages were as follows: between eighteen and twenty, *seven;* between twenty and twenty-five, *twenty-four;* between twenty-five and thirty, *seventeen;* between thirty and thirty-five, *thirteen;* between thirty-five and forty, *six;* between forty and forty-five, *three;* between forty-five and forty-nine, *two;* sixty-one of them were less than forty years of age, and only eleven older than that. The youngest case was eighteen, and the oldest forty-nine. But it may be justly objected against statistics of this sort which are drawn from military sources, that in respect to age, soldiers do not represent the average of the population, since neither the very young nor the aged are enrolled in the army to any considerable extent. I have therefore collected from various sources thirty-six cases of osteo-myelitis that occurred in civil practice, which are not open to the objection mentioned above. An analysis of their ages yields the following results: between ten and fifteen, *five;* between fifteen and twenty-five, *fourteen;* between twenty-five and thirty-five, *five;* between thirty-five and forty-five, *one;* between forty-five and fifty-five, *two;* between fifty-five and sixty-five, *one;* between sixty-five and seventy-five, *one;* and in *seven* instances the age was not stated. It will thus be seen that of the twenty-nine cases whereof the ages are given, nineteen, or about two-thirds, were less than twenty-five years old, and that in only five instances were the subjects more than thirty-five years of age. The youngest case was aged eleven, and the oldest seventy years.

Furthermore, osteo-myelitis is pre-eminently a disease not only of the period of youth, but also of the *male sex.* Of the thirty-six cases mentioned above as occurring in civil practice, twenty-seven were males, eight females, and in one instance the sex was not stated.

Pathological Anatomy of Osteo-Myelitis.—The morbid conditions of the marrow in respect to structure which are produced by the inflammatory process, are threefold: 1st. *carnification* or *hepatization;* 2d. *suppuration;* and 3d. *gangrene.*

1st. *Of the Carnified or Hepatized Marrow.*—The medullary tissue in the long bones of the adult, when healthy, is loaded

with fat, and on that account presents a pale yellow color. Among the first effects of the inflammatory process upon this tissue are a reddening of its hue, and an increase in its density and tenacity. The color of the marrow during the first stage of osteo-myelitis varies from coppery red to crimson, to brown, and almost to black. In this form, the inflamed marrow is said to be *carnified* or *hepatized*, on account of the resemblance in appearance which it is supposed to bear to red muscular tissue or flesh, on the one hand, and dark-red or reddish-brown hepatic tissue on the other. The inflammatory process produces this change in the color of medullary tissue by the following means:

1st. By diminishing the quantity of the fat, to which the yellow hue of the healthy adult marrow in the long bones must be ascribed.

2d. By increasing the quantity of one of the cellular elements of the medullary tissue, namely, the marrow cells, which, under such circumstances, are more or less granular, and resemble closely, if they are not identical with, what were formerly called exudation cells.

3d. By increasing the quantity of blood contained in the inflamed tissue, thus producing an active hyperæmia, which is proved by the fact that the inflamed marrow bleeds readily on touching it.

When the carnified marrow has a yellowish red, or a copper-colored hue, it is because fat vesicles in considerable quantity still constitute one of its histological elements. As muscular tissue acquires a yellowish tinge from undergoing the fatty degeneration, even so the inflamed marrow may retain some of that tint, because the oil has not been sufficiently expelled from it by the inflammatory process. Other things being equal, the greater the proportion of oil contained in the inflamed marrow, the deeper the yellowish tinge is found to be. But even when the carnified or hepatized marrow has a deep red, or brownish hue, the examination of it with the microscope shows, that although the proportion of fat vesicles is very much reduced, still the fat has not entirely disappeared, provided the specimen examined belongs to an adult, and has been taken from a long bone. Such at least have uniformly been the results of our own observations.

The extent to which the quantity of blood in the inflamed marrow is increased above the healthy standard, or, in other words, the amount of the congestion, has also an important bearing upon the morbid coloration of the medullary tissue.

What occasions the increase in density, consistence, and tenacity, which characterizes the carnified or hepatized marrow? It seems to be produced, at least to some extent, by the development under inflammatory irritation of a rudimentary fibrous material, represented, when examined under the microscope, for the most part, by elongated and spindle-shaped fibre cells. But this new growth of fibrous tissue has generally appeared to me to be quite insufficient in quantity to account satisfactorily for the amount of the increase in the density and consistence of the diseased marrow, and I have therefore been compelled to seek some other cause which may assist in producing it. Such a cause is probably constituted by *sclerosis* of the intercellular substance, or, in other words, *sclerosis* of the substance which lies between the free marrow cells and connects them together. The induration of the intercellular substance in such cases appears to be a result of the inflammatory process, and to depend upon the grade or degree of the inflammatory irritation. For if, from any cause, the intensity of the inflammatory irritation be sufficiently increased, the intercellular substance will soften and liquefy, there will also be a very rapid production or proliferation of the cells, and suppuration will be established; or, in other words, the carnified medullary tissue will be directly transformed into pus through the agency of a high grade of inflammatory irritation.

2d. *Of Suppuration of the Marrow.*—Carnification or hepatization appears to be the result of the *first* stage of inflammation of the medullary tissue, and suppuration to be the result of the *second* stage of that disease. Before the marrow suppurates it always undergoes the process of carnification or hepatization. Hence it happens that the abscesses produced by suppurative osteo-myelitis, in the shafts of the long bones, are surrounded by carnified or hepatized medullary tissue. Hence it also happens that as these abscesses increase in size, it is constantly done at the expense of the carnified or hepatized tissue, and by its transformation into purulent matter in the way just

pointed out. We say, again, that the transformation of new medullary growths into pus is attended by the following phenomena. The granular medullary cells multiply themselves with very great rapidity, and become still more granular. At the same time the intercellular substance softens and finally liquefies, the marrow cells become converted into pus corpuscles, and the intercellular substance into *liquor puris*, or the liquid intercellular material of purulent matter. Some of our own observations strongly corroborate these views of Virchow. We say, then, that carnification, or hepatization and suppuration of the medullary tissue, are successive stages in the transformations of the histological elements of that tissue, which are produced by the inflammatory process. Hence, it comes to pass, that we not unfrequently find, in stump-bones affected with osteo-myelitis, the marrow in a state of suppuration at the lower end where the morbid process began, and above that in a state of carnification or hepatization.

3d. *Of Gangrene of the Marrow.*—One grade of inflammatory irritation produces carnification or hepatization of the medullary tissue; another grade suppuration; and a still higher degree of inflammatory irritation produces gangrene of that tissue. When the marrow mortifies, its color usually becomes very dark or nearly black, and it emits an offensive gangrenous odor. When the mortified medullary tissue is examined with the microscope, it is found that all the cell-structures are destroyed, that all the fat vesicles are broken up, and the oil which they contained is set free. The fibres of connective tissue, however, may withstand the putrefactive process for some time; and we therefore generally find the gangrenous marrow to consist of connective tissue, free oil, and granular matter, all stained with decomposing hæmatoidin.

Robin mentions a pathological condition of the marrow which is closely allied to gangrene, and has an important bearing on the prognosis of the case in which it occurs. I have seen it several times, and I doubt not that it has attracted the attention of most surgeons. He says: "In some conditions the inflammation of the marrow becomes so intense, that the medullary cells cease to receive materials fit for their continuous molecular renovation. *The marrow then softens and*

becomes liquid, and flows from the extremity of the fractured or amputated bone. When this liquid is examined, there are found in it only molecular granules in suspension, sometimes nucleated medullary cells, and always drops of oil; for the liquefaction melts down the walls of the fat vesicles and sets the oil free. This is always a grave occurrence, as has been demonstrated in the study of fractures, especially those of the long bones, and in certain other pathological conditions, such as amputation followed by so-called purulent infection."—Vide *Am. Journal Med. Sciences, Oct.*, 1865, *p.* 498 *et seq.*

We sometimes find all three of the pathological conditions of the marrow which may be produced by the inflammatory process, present in the same specimen. Thus, I have seen a stump-bone affected with osteo-myelitis, in the lower part of which the marrow was gangrenous; above that, in the stage of suppuration; and still higher up, in the stage of carnification or hepatization.

Symptoms and Diagnosis of Osteo-Myelitis.—The first sign which announces that the medullary tissue has been attacked by inflammation is, for the most part, local pain. It usually appears to the patient to be situated in the bone at the place where the marrow is diseased. It varies much in character and intensity in different cases. In some instances, it seems to be dull, heavy, and aching; in others, it appears to be gnawing; and in still other instances, it is stabbing in character. Sometimes it is but slight in degree, while in other cases it is very intense. Occasionally, however, it is not present, or at least the patient, when asked if he has pain, always replies in the negative. This circumstance in some cases appears to be due to the hebetude of typhoid disease, in others to extreme debility; but in other instances cannot be accounted for in the present state of our knowledge of the subject.*

* The pain produced by osteo-myelitis is more likely to be intense if the disease involve a bone whose continuity has not been destroyed by amputation, resection, or fracture. Thus, I have seen a case of osteo-myelitis of the tibia which was caused by a contused wound of that bone, wherein the pain was very severe—almost agonizing. In some cases of inflammation of the medullary tissue of fractured, and likewise of stump bones, that have come under my observation, the pain

To the local pain is generally superadded local swelling as a symptom of osteo-myelitis, which appears not immediately, but after the lapse of a period varying from a few hours to several days or even weeks in duration. The tumefaction is usually more or less indurated in feel, puffy in appearance, and pits under pressure. It is generally more or less flattened in shape, and coëxtensive with the suppuration of the medullary tissue lying beneath it, at a line corresponding with the boundaries of which it usually terminates abruptly. The "puffy tumor" of Pott affords a good illustration of the various features which characterize the swelling of the soft parts produced by osteo-myelitis.

When this disease invades the stumps of amputated limbs, it is generally found that carnified or hepatized medullary tissue protrudes from the canal at the sawn end of the bone, and after the lapse of a few days, pus will not unfrequently be seen exuding guttatim from this portion of the inflamed marrow.*

Furthermore, as the stump becomes tumefied in consequence of inflammation of the medullary tissue of its bone, the purulent discharge from its granulating surface generally exhibits considerable alteration. It usually becomes considerably diminished in quantity, and thin, flaky, and serous in quality. Sometimes it becomes changed to a scanty quantity of bloody serum. Occasionally it is completely arrested, and the stump presents a dry and pork-like appearance.

When acute osteo-myelitis invades the cranium or any of the limbs in their continuity, a collection of purulent matter is not unfrequently formed in relation with the exterior of the bone at the seat of the disease. Now, on incising this abscess,

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was slight, apparently because the purulent matter could readily escape from the medullary canal.

Again, the pain which accompanies osteo-myelitis is generally very intense if the neighboring osseous tissue also is inflamed; for in such case there is superadded to the pain pertaining to the inflammation of the medullary tissue, that which is produced by the ostitis.

* If the marrow liquefies and the fat vesicles burst on account of the intensity of the inflammation, as pointed out by *Robin*, then free oil will be found flowing out of the medullary canal at the end of the *stump-bone* in a case of amputation, or from the wound of the soft parts, mixed with purulent matter in a case of compound fracture.

the matter as it flows away will often be found to contain numerous oil-globules. This circumstance denotes that the disease is either acute osteo-myelitis, or acute periosteal abscess, and is of great value in a diagnostic point of view. Chassaignac asserts that the sub-aponeurotic pus in cases of osteo-myelitis is *always* mixed with oil-globules. While I am not prepared to yield full assent to this statement, I am ready to say that I am fully satisfied, from my own experience, that the sub-aponeurotic pus of osteo-myelitis is *very often* mixed with oil-globules.

The only diseases which are likely to be mistaken for acute osteo-myelitis, are *acute periosteal abscess*, and *diffuse cellulitis* or *phlegmonous erysipelas*.

With regard to the diagnosis of osteo-myelitis itself, irrespective of any other disorder, we have to say that the only signs which can be properly considered as pathognomonic of that disease are the reddened and sclerosed or hepatized appearance of the marrow, with perhaps some drops of pus exuding from it, which are sometimes detected by ocular examination. But the clinical observation of all such signs is practically restricted to the cases wherein osteo-myelitis follows amputation, with the addition, perhaps, of some instances of resection. Now, in view of the great value of these signs, and in view of the great danger which always attends acute suppurative osteo-myelitis, it is advisable for the surgeon to open the stump sufficiently far to permit him to ascertain the condition of the marrow at the sawn end of the bone, in every case where there is good reason to suspect the presence of the disease in that form.

But with regard to osteo-myelitis produced by compound fracture, by contusion, by scrofula, by syphilis, by rheumatism, and indeed by any cause besides amputation, there are no individual symptoms of which the surgeon can avail himself that can be considered as diagnostic of the disease, especially so far as the long bones of the extremities are concerned. In cases of cranial osteo-myelitis, however, the "puffy tumor" of Pott may be considered as characteristic of the affection.

Osteo-myelitis affecting the bones of the extremities can generally be distinguished from *phlegmonous erysipelas* by attending to the following points: 1. The pain produced by the

former is usually much more severe than that occasioned by the latter disease; 2. The character of the pain is different in the two diseases; that of osteo-myelitis being mostly of an aching and gnawing character, and that of phlegmonous erysipelas being throbbing and burning, and much less intense; 3. The swelling produced by phlegmonous erysipelas is more boggy than that occasioned by osteo-myelitis; 4. The swelling produced by the former differs considerably in shape from that occasioned by the latter disease; the tumefaction resulting from osteo-myelitis terminates by an abrupt rim or border which corresponds with the boundaries of the suppurating portion of the marrow; but the swelling occasioned by phlegmonous erysipelas does not terminate in that way, since it usually sinks down gradually until it is lost in the surrounding parts; 5. If an explorative incision be made to sufficient depth it will be found that the sub-aponeurotic pus does not contain oil-globules in cases of phlegmonous erysipelas, while it generally does contain oil-globules in cases of osteo-myelitis; 6. If evacuative incisions be made to sufficient extent, they will afford great relief from the pain and distress in cases of phlegmonous erysipelas, but not in cases of osteo-myelitis.

The last-named can oftentimes be distinguished from the first-named disease by the shape of the swelling alone; which in osteo-myelitis terminates by an abrupt rim or border corresponding with the limits of the suppuration of the medullary tissue, and in phlegmonous erysipelas does not terminate in that way, as has already been pointed out.

Osteo-myelitis can be distinguished from *acute periosteal abscess* by the following circumstances: 1. In acute periosteal abscess fluctuation precedes puffiness in the swelling; in osteo-myelitis it is just the contrary (Chassaignac); 2. The painful œdema which accompanies osteo-myelitis terminates abruptly by a projecting and hard brim, just at the height where the bone ceases to be diseased (Chassaignac); 3. Osteo-myelitis may be accompanied by suppurative inflammation of the periosteum, and diffuse inflammation of the cellular tissue; the acute periosteal abscess occasions neither medullary suppuration of the bone, nor purulent infiltration of the limb (Chassaignac); 4. Osteo-myelitis spreads from one bone to another in a direction

upwards and towards the root of the limbs; the acute periosteal abscess generally remains confined to the part of a limb in which it appeared (Chassaignac); 5. If evacuative incisions are made in cases of acute periosteal abscess, they generally afford much relief from pain and distress; not so, however, in cases of osteo-myelitis.

Again, acute suppurative osteo-myelitis may sometimes be distinguished from acute periosteal abscess by still another sign—one that is founded on the condition of the bone that underlies the suppurating periosteum, which is generally necrosed in such cases, and oftentimes to great extent. Now, if the death of the bone have been occasioned by osteo-myelitis, and the suppuration of the periosteum and the neighboring parts be secondary in character, and produced by extension of the inflammatory process to them from the diseased marrow, then the surface of the dead bone is apt to present a rough, eroded, or worm-eaten appearance, occasioned by the adhesion of living particles of osseous tissue to the periosteum at the time of its separation from the dead osseous tissue; but if the necrosis be occasioned primarily by diffuse periostitis, then the surface of the dead bone is found to be comparatively smooth, the longitudinal furrows are perhaps somewhat deeper and wider than natural, but the circumferential laminæ are not eroded.

If there be any doubt concerning the diagnosis as between osteo-myelitis, acute periosteal abscess, and phlegmonous erysipelas, it is generally advisable to make an explorative incision down to the bone, for the purpose of gaining such information as will settle the question definitely; for even extensive incisions made in such cases do not do harm, but, on the contrary, usually prove beneficial.

If for any reason it be deemed inexpedient to make explorative incisions for the purpose of verifying the diagnosis, much valuable information may not unfrequently be obtained by using the exploring needle, in cases where the periosteum is detached and separated from the bone by a quantity of pus. If in such a case the needle be thrust into the swelling in a direction towards the bone, and it be attempted to move it in circular sweeps, an equable resistance will be found as long as

it remains in the soft parts; but if it be thrust down to the bone, and then drawn back a little (about a line), its point may be turned in any direction. (Barwell.)

If, when purulent matter has been withdrawn from deep-seated parts either by incision, or through an exploring canula, or by spontaneous discharge, there be doubt as to whether its production is dependent upon disease of the osseous tissue, the question may not unfrequently be put to rest by finding in the pus particles of disintegrated bone resembling fine sand, which, on examination with the microscope, are found to contain lacunæ, canaliculi, and Haversian canals; and by finding in the purulent matter, on the application of chemical tests, the calcareous salts belonging to bone, in at least considerable quantity.

But, while endeavoring to establish the diagnosis in an acute case of suspected osteo-myelitis, it is a matter of the first importance not only to interpret each symptom with rigidity, but also to consider the *tout-ensemble* of the morbid phenomena, or to give attention to the symptoms as a whole; and while so doing, the method of exclusion may be advantageously employed. If all the symptoms can be satisfactorily accounted for on the hypothesis that the disease is osteo-myelitis, and not by any other hypothesis, then it is certain that the case consists of that disorder.

The diagnosis of *chronic osteo-myelitis* is, in general, not difficult to make. The clinical history of the case; the length of time it has lasted; the order in which the morbid phenomena have been developed; the bone-pain, the heat, the swelling, together with the suppuration of the soft parts; and, in patients who have previously suffered a solution of the continuity of the diseased bone either by fracture, or resection, or amputation, the occurrence of necrosis with the development of new osseous tissue from the periosteum and from the portion of the bone that did not lose its vitality, which, for the most part, can be readily ascertained; the persistent suppuration of the parts contiguous to the sequestrum, resulting from the mechanical irritation occasioned by its presence; and, perhaps, the exfoliation from time to time of fragments of necrosed bone—do not leave us long in doubt with regard to the real nature of the disease.

TERMINATIONS, COMPLICATIONS, AND CONSEQUENCES OF OSTEO-MYELITIS.—Here time permits us to state but little more than the results of our investigations. It forbids us to relate in detail the cases, the pathological researches and arguments, upon which our conclusions are founded.

1st. Osteo-myelitis, even when acute, not unfrequently terminates in *recovery by resolution.* Of this I have seen two notable instances. In such cases the inflammatory irritation subsides, the symptoms which indicate to the clinical observer the presence of the inflammatory process in the marrow disappear, and the diseased tissue is gradually restored to a healthy condition, without the intervention of suppuration. But the acute variety of the disease much more frequently degenerates into a chronic form, which is, for the most part, susceptible of relief by surgical means.

2d. Osteo-myelitis often occasions *central necrosis.* The army medical museum at Washington contains a large number of specimens belonging to this category. When the disease affects the shafts of the long bones, the internal laminæ, that is, the laminæ which lie next to the marrow, may become necrosed, or, in other words, affected with dry gangrene (for necrosis is in reality dry gangrene of the osseous tissue), by being deprived of an adequate supply of blood because of the morbid condition or destruction of the medullary tissue. When the disease affects the epiphyses of the long bones, it may occasion necrosis of a portion of the cancellous structure—indeed, it may destroy the vitality of any part of the osseous structure which lies within the shell formed by the external layers of compact tissue.

3d. The inflammatory process may spread from the marrow to the osseous tissue and periosteum, and thus occasion necrosis of the whole thickness of the bone, or *necrosis in totality.*

4th. Osteo-myelitis not unfrequently produces *pyarthrosis.* In fifty-one cases of said disease, suppurative arthritis is known to have occurred in five instances. As already stated, inflammation of the medullary tissue exhibits a strong tendency to travel upwards from one bone to another, towards the roots of the limbs, and in this way invades the neighboring or inter-

vening articulations. Thus, when the disease commences in the bones of the tarsus, it not unfrequently occasions pyarthrosis of the ankle; when it begins in the tibia and spreads upwards, it produces pyarthrosis of the knee; and when it originates in the femur, it often occasions pyarthrosis of the hip. Oftentimes the morbid process appears to enter the joint by perforating the cartilage of incrustation, which then contains a number of small reddish-colored holes, that look as if they had been made with a punch. In the specimens I have examined, the cancelli underlying these holes were found to contain purulent matter, that is, matter containing pus-corpuscles.

5th. Osteo-myelitis may occur simultaneously in more than one bone. Stanley and Chassaignac have each reported a case. The writer has seen one instance.

6th. Osteo-myelitis very often occasions the *development of new osseous tissue*, which may be formed either in connexion with the medullary tissue, when the new bony growth is called *endostosis*; or in relation with the periosteum, when it is called *periostosis*; or in the connective tissue of a limb, when it is known by the name of *osteoid tissue*.

7th. Osteo-myelitis frequently produces *abscesses in the neighboring soft parts*, of which we have observed numerous examples.

8th. Osteo-myelitis not unfrequently causes the blood to coagulate in the veins leading from the affected part, thus producing thrombi, and inaugurating the peculiar morbid process which Virchow has called *thrombosis*. In fifty-one cases of this disorder, six are known to have had thrombosis. Several of them had parenchymatous hæmorrhage in consequence of the venous obstruction. In one of them it was very profuse, and proved fatal. This patient did not have pyæmia.

9th. Osteo-myelitis often occasions *pyæmia*. Of the fifty-one cases of inflammation of the medullary tissue, pyæmia supervened in eighteen, and all of them died. In each of these cases the suppurating marrow appears to have been the focus from which the purulent infection sprang. Furthermore, in cases of osteo-myelitis having a fatal termination, pyæmia is the immediate cause of death more frequently than all other causes combined. Thus of fifty-one cases of osteo-

myelitis, thirty proved fatal; and of these, eighteen, or considerably more than one-half, died of pyæmia. Hence, it is believed that pyæmia is the form of systemic intoxication which is produced by suppurative inflammation of the marrow. Clinical observation has also abundantly shown that purulent infection occurs in surgical cases but seldom, speaking comparatively, unless the osseous tissue is injured.

10th. Osteo-myelitis is sometimes complicated with *leukæmia*, of which I have seen one remarkable instance.

11th. Osteo-myelitis not unfrequently occasions death by *exhaustion*. Of thirty fatal cases of said disease, six died from that cause.

But the closeness of the relationship which exists between suppurative inflammation of the medullary tissue of bone and pyæmia is very remarkable. No such relationship obtains between suppurative inflammation of any other tissue in the whole body and that disease. It is but seldom that we meet with pyæmia in a severe form in military practice, unless it has followed a traumatic lesion of the osseous tissue, or a surgical operation rendered necessary by such lesion. The writer has seen only two cases of pyæmia in military practice which were entirely disconnected with injury of bone, among a large number of instances of that disease.

Now, this question naturally arises, namely: Why is it that purulent infection is very apt to follow after suppurative inflammation involving the medullary tissue of the femur, for example, and almost never occurs in connexion with suppurative inflammation of the muscular and connective tissues of the thigh, unless a lesion of the osseous tissue also is present? In answer to this question it has been suggested, that the patulous condition of the veins in the osseous tissue and the marrow itself occasions the difference. It is believed that the patulous condition of the veins belonging to those tissues facilitates the absorption of the liquor puris, and the contamination of the system at large with a purulent infection.

Treatment of Osteo-Myelitis.—The treatment of this disease must be considered in respect to that which is best

adapted to procure relief from the acute and chronic varieties of it respectively.

1st. The *prophylaxis* and the *hygienic treatment* of osteo-myelitis constitute a subject of very great importance. The chief objects to be accomplished by the prophylactic treatment, in cases of injured bones, are to prevent a simple formative irritation of the marrow from becoming an inflammatory irritation; and if, perchance, the inflammatory process has been excited in the medullary tissue, to prevent it from running into the stage of suppuration; and if, again, suppuration has been established, to prevent it from degenerating into the putrid forms thereof, which are almost certain to induce pyæmia, and thus occasion the death of the subject. The elements of the hygienic treatment of osteo-myelitis consist in placing the patient beyond the reach of certain deleterious influences, which tend to impair the forces of his system and to poison his circulating fluids, and thereby convert a simple into a destructive osteo-myelitis, and finally produce death by pyæmia. The deleterious agencies of which the hygienic or prophylactic treatment takes cognisance, are chiefly foul air, especially the foul air of imperfectly ventilated hospitals, the overcrowding of patients, especially those who are wounded, and want of cleanliness. The principles of the hygienic treatment require that the wounded, especially such of them as have sustained lesions of the osseous tissue from any cause whatever, should not be treated in the wards of imperfectly ventilated hospitals; that it is much preferable to place them in hospital tents; that an air-space of at least twelve hundred cubic feet should be allowed each patient, even in well ventilated hospitals, that have been constructed with special reference to the care of the wounded; that an allowance of less than twelve hundred cubic feet of air-space to each patient (I am speaking of the wounded) amounts practically to overcrowding; that the hospital quarters, utensils, bedding, clothing, etc., and the person and wounds of the patient, should be kept scrupulously clean; and that the patient himself should be kept in a quiet and composed state of mind and body. The air of the ward or apartment in which the wounded are treated should not be allowed to become contaminated with the effluvia arising from suppurating wounds.

To prevent such an occurrence, the ventilating apparatus should be so constructed as to furnish a constant and abundant supply of pure atmospheric air. It should have the capacity of renewing all the air of the ward as often as once in every hour, at least, if necessary, and the supply should be constant and equable.

2d. *Internal Medication.*—Since osteo-myelitis is, for the most part, a disease of debility, the internal treatment should partake of a supporting, tonic, and soothing character. The diet should generally be nourishing and easy of assimilation, and care should be taken that it is sufficient in quantity. It should consist very largely of strong beef-tea, or strong broths made from other forms of animal food. Both the mineral and the vegetable tonics have been found serviceable in this disease. Among the former the ferruginous preparations, such as tinct. ferri muriat., and ferri et potassæ tart., and among the latter quinise sulphas, have appeared to be the most useful. The citrate of iron and quinine is a convenient remedy. When the inflammation of the medullary tissue was complicated with a scorbutic taint, as it not unfrequently is in military practice, the tartrate of iron and potassa was administered with very good results. In some cases of this disease which I have seen in military practice, the debility or the depression of the systemic forces has been so great as to require the administration of alcoholic stimulants in the form of ale, porter, wine, milk-punch, etc., together with nutrients and tonics, from the outset of the attack. For the alleviation of pain, which, in both the acute and chronic varieties of this disease, is often very considerable, it has been found necessary to give anodynes, of which the various salts of morphia, especially when administered hypodermically, have proved extremely valuable. The febrile movement which is always present in the acute variety of the disease, oftentimes renders it advisable to prescribe aconite, with febrifuge remedies such as mistura neutralis, spts. mindereri, etc. Stanley recommends that in cases of acute inflammation of the medullary tissue, mercury should be administered internally, "in the view of producing its full influence upon the system." (Op. cit., p. 45.) Now, inasmuch as this affection is one which belongs especially to debilitated subjects, and

is also essentially a disease of debility, the propriety of administering mercury to the extent of ptyalism in its treatment is, to say the least, questionable. I have never seen a case of osteo-myelitis which would, in my opinion, have been benefited by a mercurial course of treatment. On the other hand, it has always seemed to me probable that such a course of medication would have done much harm to the subjects of that disease which have come under my observation. Indeed, I should as soon think of ptyalizing a patient affected with tubercular pneumonia, in order to cure his disease, as to ptyalize the patients affected with osteo-myelitis that have come under my notice. There is, however, one remedy which can often be administered, in the subacute and chronic forms of the disease, with much benefit, and that remedy is the iodide of potassium. It generally proves most serviceable in cases where nocturnal bone-pains are present. In some cases it is necessary to administer large doses of this remedy in order to obtain the relief desired. It is therefore a good plan to give this medicine in increasing doses in all obstinate cases of the disease wherein its administration is indicated.

The *rheumatic*, the *syphilitic*, and the *scrofulous* types of osteo-myelitis, require internal medication appropriate for the removal of the special diathesis upon which the local affection depends.

3d. *Topical Medication.*—The part affected with acute osteo-myelitis should be placed in such a position as does not favor the stagnation of blood in its vessels; that is, it should be placed in an elevated position, and should be kept in a state of perfect rest.

If the severity of the attack be very great, and the patient's strength considerable, it may be advisable to abstract blood locally with leeches or cups. This means of subduing inflammation should not be overlooked, especially when the disease appears in a sthenic form.

In most cases of acute inflammation of the medullary tissue, the ice-dressing can be applied to the diseased part with benefit. Should the ice-dressing prove to be uncomfortable, because of its weight, the cold irrigation can be employed in its stead. Should the cold applications cause the patient to be chilly and

to feel disagreeably, warm applications, such as the warm-water dressing, should be substituted for them; and, generally, the patient's sensations will afford a guide that is sufficiently correct with regard to the use of cold, tepid, or warm applications, in the treatment of the acute variety of this disease.

In the subacute and chronic forms of osteo-myelitis, the tincture of iodine can often be applied to the affected part with marked advantage. It should be applied daily, and over a large extent of surface.

4th. *Operative Medication.*—Incisions into the inflamed soft parts down to the bone may often be advantageously made in treating both the acute and chronic varieties of this disease, for a considerable amount of local depletion is always obtained in this way. Furthermore, they are always required when accumulations of matter (abscesses) are formed, and for the relief of such a condition should be made at an early period—indeed, the earlier the better after the presence of pus is once detected. Osteo-myelitis, when it occurs spontaneously, is very apt to be accompanied by diffuse inflammation of the soft parts, which, to a greater or less extent, masks the real nature of the disease. In all such cases, free incisions carried down to the bone prove decidedly beneficial. They relieve the painful tension of the soft parts immediately, and, at the same time, evacuate any purulent matter which may have collected between the detached periosteum and the bone. Even in cases where the diagnosis is doubtful, and the soft parts are in a state of diffuse inflammation, it is considered justifiable practice to cut down to the bone in order to ascertain the condition of the periosteum, and whether it is detached, and likewise the condition and appearance of the bone, and whether it is necrosed, for by so doing the diagnosis can be settled.

The operation of *trephining* the diseased bone may sometimes be advantageously practised in cases of osteo-myelitis. In that form of chronic inflammation of the medullary tissue, which is called chronic abscess of bone, the evacuation of the purulent matter, and the removal of the diseased tissue by means of this operation, afford the only means of obtaining a cure short of amputation. Thus, *Duverney* states that in two cases where matter was imprisoned in the bony tissue, it was

necessary to perforate with a trephine. (Vide *Traité pratique d'Anatomie Medico-Chirurgicale, par A. Richet, p.* 61. Paris, 1860.) *Hey*, as we have already shown, performed an analogous operation upon the tibia in two instances of abscess of the medullary canal, with a good result in both of them. It also appears that the trephine has sometimes been usefully employed in treating inflammation of the medullary tissue, having a more acute form. *Mr. Abernethy* states in his Surgical Observations, that he has seen several cases of suppuration in the diploë of the cranium, where, the trephine having been applied early, the external table of the skull came away in the crown of the instrument; "the matter was discharged from the medullary part of the bone, and the internal table remained sound and entire, covering the dura mater. Granulations soon arose, and the patients got well with the exfoliation only of a portion of the outer table." (Vide *op. cit.*, Vol. II.; also Stanley on the Bones, p. 59.)

Again, excision of the diseased part by surgical operation, not unfrequently becomes necessary in the treatment of osteo-myelitis involving the bones of the extremities. Thus, of seven cases of that disease reported by *Mr. Stanley*, three were treated successfully by amputation. The arm was amputated in one instance, and the thigh in the other two. One case recovered without the performance of any operation. In it a large part of the tibia exfoliated, and the knee-joint became permanently anchylosed. In this case, also, the pain, which was very great, and the inflammatory fever which ran very high and was accompanied by delirium, were all relieved by the bursting of a large abscess a little below the knee. Could not relief from these distressing symptoms have been obtained at an earlier date by making free incisions? In the remaining three of Mr. Stanley's cases no operation could be practised, and they proved fatal.

Limbs affected with destructive osteo-myelitis may be excised either by amputation or by exarticulation. *Mr. Stanley*, as we have just seen, employed the operation of amputation. *Dr. T. Vallette*, however, finding all his attempts to save patients affected with acute osteo-myelitis following gunshot fracture of the long bones, in the Crimean war, by secondary

amputation, performed in the continuity of the limb, ineffectual, recommended that amputation should be abandoned in the treatment of such cases, and that exarticulation of the diseased bone should be practised in its stead. *Dr. Jules Roux*, at the hospital of St. Mandrier, at Toulon, also found that he could not save his osteo-myelitic patients (they were soldiers who had been wounded in the Italian campaign of 1859) by secondary amputations, and therefore put into practice the recommendation of Vallette. He achieved a remarkable success thereby; for in twenty-two successive cases wherein the operation of exarticulation was performed for osteo-myelitis, no death occurred. Moreover, four of these exarticulations were performed at the hip.

The operation of exarticulation presents the following advantages over amputation performed in the continuity of the diseased bone in cases of osteo-myelitis: 1st. It effects the removal of the whole of the diseased medullary tissue, while amputation in the continuity does not. Now, the inflamed medullary tissue which is left behind in the stump-bone by amputation performed in such cases, proceeds unchecked to the stage of suppuration and ultimately destroys the patient by inducing pyæmia, with about the same certainty that it would if no operation had been performed, of which I have seen at least one remarkable instance, and have had the pathological specimens belonging to the case figured in colors, after nature. 2d. The operation of exarticulation does not expose the medullary tissue to injury; and 3d, A longer stump can be obtained for the patient in this way than by amputation performed in the continuity of the sound part of the limb. The operation of exarticulation has already been performed several times in this city for osteo-myelitis. Thus, *Dr. Weir* has exarticulated the thigh at the hip, for chronic osteo-myelitis of the femur following gunshot fracture with a good result, at St. Luke's Hospital. *Prof. Van Buren* exarticulated the leg at the knee for acute osteo-myelitis of the tibia last March, in private practice, with a good result also; and recently, *Prof. Hamilton* has exarticulated the thigh at the hip, for chronic osteo-myelitis of the femur, at Bellevue Hospital. In this case, the disease occurred spontaneously. The operation was not attended with

shock. The patient did well for a period of two weeks, but ultimately died with symptoms of pyæmia.

Chronic osteo-myelitis involving the stumps of amputated limbs, often produces morbid conditions which require operative interference. For example, it often occasions deep-seated abscesses which demand early and free incision. Furthermore, osteo-myelitis, in its subacute and chronic forms, is very apt to occasion *necrosis of stump-bones.* In most instances belonging to this category, the necrosis is limited to the lower portion of such bones; but occasionally it involves the whole of the shaft up to the superior epiphysis. Many cases of necrosis of stump-bones, produced by osteo-myelitis, were witnessed in our military hospitals during the late war. The operative treatment usually employed consisted in the extraction of the necrosed portion of bone from the stump by surgical means, as soon as it had become detached from the living osseous tissue. For this purpose incisions were made in the end of the stump to such extent and in such directions as might be necessary in order to effect the liberation of the dead bone, which was then generally extracted with necrosis forceps without further difficulty. The result of such cases was almost invariably favorable.

In concluding this memoir, the writer desires to say that it has, in the main, been drawn up in connexion with an extended report on the subject of osteo-myelitis, which he has prepared at the instance of the U. S. Sanitary Commission; and that the clinical observations, pathological investigations, anatomical facts, and arguments upon which the conclusions exhibited in this memoir are founded, are stated at length in said report.

332 Sixth Avenue, New York, November 8, 1866.

ON THE

ETIOLOGY OF BRIGHT'S DISEASE,

WITH REMARKS ON THE PROPHYLAXIS.

Read before the New York Academy of Medicine, April 1, 1869.

BY

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MR. PRESIDENT AND FELLOWS OF THE NEW YORK ACADEMY OF MEDICINE—The year 1862 will ever be a memorable period in the history of this Academy. In that year the subject of Bright's disease here received special attention. The etiology, symptomatology, pathology, and therapeutics of the malady, each in turn and on successive occasions, were thoroughly treated of in an original manner by masterly experts appointed by the Academy to fulfil the duty. Since that time no material advance has been made in our knowledge of the therapeutics of the disease. The views then set forth regarding its pathology have been perhaps somewhat modified, and its etiology now appears in a clearer light.

With an unpromising future in reference to any improvement in our method of treating Bright's disease, it seems necessarily incumbent upon us to study more closely its etiology, to seek more diligently for its prophylaxis, and at the same time not to intermit any efforts to find further means for its palliation. In such investigations we are greatly facilitated by an acquaintance with its pathology, and consequently I shall refer to this subject to illustrate the topics under special consideration this evening.

No enthusiasm displayed by us in elucidating etiology and prophylaxis can eclipse that manifested by our forefathers in the profession. The illustrious and lamented Rush anticipated that the time would come when his successors might exult in the perfection of their science. "Hospitals," said he, "shall be unknown. The groans of pain, the ravings of madness, and the sighs of melancholy shall be heard no more. The cradle and the grave shall then no longer be related!"

This utterance can scarcely appear so utopian to us as it must have seemed when he gave it expression. How readily can we now destroy the germs of impending epidemics, which in his day would have been sown broadcast, and multiplied and have desolated the land. But it is unnecessary to dwell upon the means of prevention of disease known at the present day, and which would have appeared chimerical in the age in which he lived.

Briefly and imperfectly I shall this evening chiefly make allusion to the etiology and prophylaxis of Bright's disease, and endeavor partially to illustrate these points by means of a few statistics relating to the malady as it has appeared in the New York Hospital. By comparing and recomparing views and experiences in regard to these and kindred topics, we may reasonably expect to reap finally, a harvest of practical information.

> "There's fruit in each wind-wafted seed
> Waiting its natal hour."

The medical records of the New York Hospital present the histories of three hundred and eight cases of Bright's disease. For convenience of description I shall simply for the present divide the disease, as ordinarily done, into two forms, viz., first the *acute* or tubal nephritis; and second the *chronic*, in which the kidneys have undergone structural alteration. As thus viewed there could be presented, if it were desirable, the symptomatology, treatment, and result of forty cases of the first variety, and two hundred and sixty-eight of the second; but avoiding as far as possible unnecessary details, I will chiefly call attention to the more important facts relating thereto.

First, in regard to the *acute* form. Of the forty cases alluded to, thirty-two were males, and seven females. The nativities

were as follows: Ireland, fifteen; United States, nine; England, seven; Germany, five; Scotland and Canada, each one; not stated, two. The occupations of the males were: sailors, seventeen; tinsmiths and carpenters, each two; miller, puddler, porter, gardener, shoemaker, painter, pocketbook-maker, soldier, and longshoreman, each one; not stated, two.

The causes assigned were: exposure to cold and wet, and checking perspiration, twenty-one; sequelæ of scarlet fever, two; sequela of malarial fever and excessive alcoholic drinking, each one; not stated, fifteen. Several of these patients were suffering also, at the time of admission, with dysentery, pneumonia, and other acute inflammatory diseases. It is noted that anasarca occurred in thirty-one cases, ascites in eleven, convulsions in one, and threatened coma in one. The average age of the patients was thirty-two years, the youngest being fourteen and the oldest fifty-six years. The average time under treatment in the hospital was thirty-four days.

The result of these cases was as follows: recovered, thirty-six; returned to friends *in statu quo*, two; died two. Of the latter, one patient aged eighteen years was admitted semi-comatose; survived four days, and died of congestion of the brain; the other, aged twenty-eight, was admitted moribund, and succumbed within twenty-four hours, autopsy exhibiting acute nephritis.

Second, in regard to the *chronic* form of Bright's disease. Of the two hundred and sixty-eight cases occurring in the hospital, two hundred and thirty-three were males and thirty-five were females.

The nativities were as follows: Ireland, ninety-three; United States, seventy-seven; Germany, forty; England, twenty-three; Sweden, six; France and Scotland, each three; Denmark, Switzerland, and Norway, each two; Prussia, Sicily, and Prince Edwards' Island, each one; not stated, fourteen.

The occupations were: sailors, ninety-six; laborers, seventeen; merchants, nine; clerks and mechanics, each eight; porters, six; sea-captains, carpenters, and bartenders, each five; carmen, stage-drivers, shoemakers, engineers, and waiters, each four; brewers, soldiers, painters, printers, grocers, and firemen, each three physicians, authors, machinists, tailors, cigar-makers, book-binders, servants, and blacksmiths, each two; and clergyman,

lawyer, chemist, milkman, planter, silver-plater, gilder, miner, butcher, and custom-house officer, of each one.

As throwing some light upon the etiology it may be stated that fifty-eight were of intemperate habits, that twenty-three attributed their disease to exposure to cold, that seven had been poisoned with malaria, that six were workers in lead, and that one was an opium eater.

Anasarca occurred in two hundred and eighteen cases, ascites in seventy-eight, convulsions in twenty-three. Fifty-seven suffered with impairment of vision, one becoming totally blind. The retinal lesion in many instances had taken place antecedent to admission, in others it gradually developed while under hospital supervision. The average age of these patients was thirty-seven years, the youngest being eleven, and the oldest seventy-one. The average time under treatment in the hospital was — days.

The result of these two hundred and sixty-eight cases of chronic Bright's disease was as follows: died, one hundred and thirty-five; discharged relieved, one hundred and seventeen; by request, fourteen; insane, two;

Of the one hundred and thirty-five cases terminating unfavorably, it should be stated that a large number were admitted moribund, or nearly so, and can hardly be said to have been under regular treatment for the disease. Several were found insensible in the streets, and brought in, while uræmic and comatose, by the police; others were brought in by friends in the last stage of the disease; others, while suffering with the chronic disorder, were brought to the hospital laboring under an intercurrent acute malady. For example, twenty-one cases were admitted *in extremis*, and died within twenty-four hours. Nine perished in two days, six in three days, four in four days, five in five days, four in six days—making a total of forty-nine cases dying within a week after their admission.

Autopsies were permitted in seventy-eight instances.

In the fifty-seven cases in which no after-death examinations were made, numerous complications were recognized. Eleven were admitted moribund; nine died uræmic and comatose; nine had congestion or œdema of the lungs; eight had hypertrophy or valvular disease of the heart· six had tuberculosis; three had

delirium tremens; and several had apoplexy, pleurisy, disease of the liver, chronic diarrhœa, chronic uterine disease, and erysipelas.

Designing this evening mainly to illustrate the etiology of the disease, I refrain from any extended analysis of the pathological lesions revealed by the autopsical examinations; and would reserve for future use, here or elsewhere, the statistics gathered on this subject. Omitting, therefore, a consideration of the varieties of renal alterations observed, I will at present merely allude to the concurrent lesions and notice a few of the more interesting pathological points connected with the cases alluded to.

In the seventy-eight post-mortems that were permitted, the following conditions were observed in addition to those strictly Brightian in character, viz.:

Cerebral meningitis, eight; spinal meningitis, one; apoplexy, three.

Hypertrophy of heart, thirty-three; valvular disease of heart, eighteen; heart flabby and pale, five; ossification of vessels, five; atheroma, thirteen; pericarditis, seventeen; endocarditis, three.

Lungs congested and pneumonic, twenty-eight; œdematous, twenty-two; tubercular, seven; emphysematous, one; pleurisy, forty.

Acute peritonitis, four; ascites, twenty-one.

Liver fatty, twenty-three; cirrhosis, eleven; abscess, one; otherwise diseased, nineteen.

Spleen enlarged, four; pancreas scirrhus, one.

Ulcerated Peyerian patches, two; ulceration large intestines, two; tubercular mesenteric glands, one.

Diabetes complicated one of these cases.

In viewing these records it is desirable to note that with scarcely an exception, the cases exhibited gave structural lesions in addition to those shown in the kidneys.

It is a matter of regret that I am not this evening prepared to present a table giving the relative frequency of each variety of casts as found in the urine and as connected with the different nephritic lesions. This may be a point worthy of future consideration. I would remark in passing, however, that the waxy casts have been observed more particularly in the urine of pa-

tients suffering with Bright's disease and with chronic suppurating sores, and the occurrence has led me to believe that the association was not a mere casual one. Their presence may generally indicate a peculiar pathological condition which hereafter will be described.

In reference to the presence of albumen and casts in the urine, it may be said that they were voided by the majority of patients. Some specimens of urine, however, contained albumen without casts; others, casts without albumen, and others again did not present either one of these morbid ingredients.

As an instance of the latter, I would cite the case of a French sailor, forty-five days under observation in the hospital, in whom the disease was fully developed, having anasarca and retinal lesion, dying uræmic and comatose, the autopsy exhibiting the right kidney of the large white variety, fatty, and weighing eight ounces; the left fatty, and weighing eight and a half ounces; also hypertrophy of the heart and fatty liver. Again, in the instance of a Scotch barkeeper, 23 days in the hospital, having œdema and ascites, dying comatose; post-mortem showing the kidneys small and contracted, weighing only six ounces; the heart large, pale, and flabby, and the liver fatty.

Occasionally albumen and casts do not show themselves until within a few days of death, as occurred recently in the case of a lady under my care in private practice, Dr. Alonzo Clark being my associate in the observation and treatment.

In reference to the presence of albumen in the urine, I would ask whether it may not be a salutary symptom under certain circumstances? May it not be an effort of nature to remove from the system a material which might be converted into an inimical substance? As the process of assimilation is disturbed in Bright's disease, the nitrogenous principles of the ingesta may not all be appropriated, and hence, if not ejected from the system, may be converted into excrementitious material, thus inducing toxæmia.

In diabetes we deprive patients of amylaceous food, giving aliment in the form of the protein compounds; as a result the sugar in the urine is diminished, as well as the excessive diuresis. In Bright's disease, if symptoms denoting unusual uræmia are present, or if threatened, we direct an opposite course; viz., a

diet of the ternary proximate principles, selecting arrow-root or rice for such purpose. Under such food, conjoined with other means suited to relieve the kidneys of congestion, the uræmia is diminished and the albumen in the urine lessens in quantity. As a rule, invalids suffering with Bright's disease are allowed more nitrogenous food than is necessary; the ordinary diet, though mixed, should predominate in saccharine, amylaceous, and fatty principles.

According to Parkes, the amount of albumen eliminated in the urine of patients affected with renal dropsy varies from five grains to nearly nine drachms.

I would briefly call attention to two experiments, made at my request by Dr. Hathaway, senior walker in the New York Hospital. The urine of two patients, suffering with the disease in question, was daily carefully examined for albumen, &c.: first while they were under a mixed diet; and second, when on an aliment chiefly composed of rice and arrow-root.

In one case the amount of albumen ordinarily excreted was small; on the third day after being placed on amylaceous food it was imperceptible; on the fourth and fifth days the albumen, though present, was merely appreciable.

In the second case the albumen daily thrown off in the renal secretion was much larger. For four days he was placed on food mainly consisting of ternary proximate principles. On each successive day the albumen was materially diminished.

Lehmann, several years since, proved how very essentially the chemical composition of food influences the excretion of urea; the elimination of this principle being at its minimum when the pabulum taken is amylaceous, saccharine, or oleaginous in character.

Guided by such facts and by clinical experience, I am led to believe that in Bright's disease, if the patient is threatened with uræmia, it is a favorable symptom if the urine is heavily loaded with albumen; a principle is rejected, which, if retained, would indirectly augment the toxæmia. It is a simpler process to passively exude this protein compound than to assimilate it, and subsequently transform tissue into excrementitious material, which the kidneys, in their diseased condition, would be unable entirely to remove.

The escape of albumen from the blood into the renal secretion may be due to mechanical causes; but nature is, to a certain extent, so conservative in her processes, that we may at times regard the transudation as salutary. There may be hydræmic conditions in which a drain of albumen from the blood is pernicious; but in the statement just made we refer to albuminuria as it occurs under certain conditions in Bright's disease.

I would not convey the idea that the loss of albumen is always desirable; on the contrary, the loss of this principle may be finally injurious. Occasion will subsequently be taken to show that this impoverishment of the blood may prove one of the causes of amyloid degeneration. Certain forms of diarrhœa may be productive of favorable results; if, however, they are unrestrained by nature or by artificial means, they may be productive of evil consequences. A similar view may be taken in regard to albuminuria—in some instances it may be beneficial, in others the reverse.

Dr. John H. Griscom, about a year since, suggested that the cause of puerperal convulsions and of Bright's disease was a condition of hyper-albuminæmia, and that the escape of albumen in the urine was to relieve such condition. It does not seem to me, however, that the phenomena observed in those maladies can thus be accounted for. The blood of patients suffering with Bright's disease, as we have before remarked, may become surcharged with albumen from improper food, and the excess may be eliminated in the urine; but albuminuria occurs long antecedent to any appreciable surplus of albumen in the blood, and continues when we know that the proportion of albumen in the circulating fluid is far below the normal standard.

The group of cases we have alluded to exhibits in a remarkable manner how suddenly Bright's disease may manifest itself in one apparently well, and how abruptly and unexpectedly it may terminate life.

As illustrative, I would refer to the case of a driver, admitted to the hospital after an illness of two days, who early suffered with anasarca, ascites, and amaurosis, and who, at the expiration of fifteen days, died comatose; post-mortem revealing granular kidneys, acute pleurisy, pericarditis, and disease of the liver. Another case terminated fatally after an illness of only seven

days' duration. Again, cases have been brought to the hospital by the police, who had fallen insensible in the street, and who suddenly succumbed without giving an opportunity of gleaning from them anything of their previous history. Doubtless in some of these cases the disease must have manifested itself in an unusually rapid manner.

It is evident that in these cases structural lesions must have occurred antecedently to the development of the symptoms indicating them; in other words, they were instances of *latent* Bright's disease. A single kidney, or a part of one or both kidneys still intact, or but partially diseased, can for a considerable season occasionally perform the eliminative duty of both kidneys. If this fraction is disturbed by the further advance of the disease, or be congested by reason of the patient's exposure to cold and wet, or by yielding to intemperance, function is diminished or arrested, and the patient is placed almost in the same condition as if he had been nephrotomized.

An opposite condition to the one I have just alluded to is sometimes scarcely less remarkable. Cases at times present themselves exhibiting the ordinary phenomena of Bright's disease, and linger through a period of weeks, and perhaps months, yet on their death a comparatively insignificant lesion of the kidneys is discoverable. In such patients, as well as in those in which there is extensive renal metamorphosis, death does not always result from what is ordinarily regarded as the strictly Brightian element of disease. Digestion is imperfectly performed and assimilation impaired, asthenia may be extreme, while at the same time the cardiac, pulmonary, and hepatic lesions may take precedence of the nephritic in importance. If in such cases the cause of death is ascribed to Bright's disease, the term should be defined as that of a hydra, viz.: a multifarious evil. Frequently also it happens that an acute malady terminates the life of a patient suffering with the disease under consideration, induced perhaps by the ureal intoxication.

Before dismissing the subject of these protean lesions I would call attention to one which occasionally occurs, and to the existence of which I do not remember to have heard allusion. *Deafness* is sometimes developed during the course of the disease. This symptom, when transient, and accompanying a lethargic

condition of the patient, has been properly attributed to uræmia. The same symptom, however, in conjunction with others, aural in character, may be permanent, in which case I ascribe their occurrence to a peculiar *lesion of the ear;* a lesion peculiar in that it holds a relationship to the nephritic disorder, and is not, so to speak, of accidental origin.

Is it not proper to view the association of the *aural and nephritic lesions* in the same light as we regard the occasional coexistence of the ocular and renal?

Bright's retina is now fully recognized by the profession. It sometimes happens, as we are all aware, that patients suffering with defective vision, on applying for relief are informed for the first time that they have a renal disease. I would ask the attention of those surgeons who are making aural affections a special study, to the opinion just expressed in reference to a Brightian lesion of the ear. It would seem desirable, when treating patients affected with obscure auditory diseases, to examine their urine both chemically and microscopically. A latent Bright's disease may thus be detected, and the cause of the aural malady thus be explained.

While I am not confident that the medical profession will accept the pathological divisions of Bright's disease as enunciated by Dickenson in his recent elaborate monograph,* I will, nevertheless, on this occasion at least, adopt his nomenclature, and endeavor to present his views, as well as those of others and my own, in regard to the topics selected for consideration this evening.

In coming now to speak of the etiology of the disease, and of such preventive measures as we at present can recognize, we should again bear in mind the varieties of the disorder. Albumen and casts in the urine may occur as a result either of tubal nephritis, or of granular degeneration, or of depurative infiltration of the kidney, and albumen may be present from various other causes.

Acute Bright's disease or tubal nephritis, the more remediable form of the malady, may be induced by a sudden congestion of the kidneys, or by an irritable condition of the urine. In the

* "On the Pathology and Treatment of Albuminuria," by William H. Dickenson, M.D., Cantab., etc. New York Edition. Wm. Wood & Co., 1868.

forty cases of this affection occurring in the New York Hospital, twenty-one were ascribed to exposure to cold and wet, and checking perspiration; two occurred as the result of scarlet fever; one subsequent to a malarial fever, and one from immoderate alcoholic drinking. I have observed it, I think, in one case as a sequel of measles. Other causes have been assigned: for example, Dickenson informs us that it may occur from obstructions to the escape of bile, and that in various diseases the urine, being irritable, may excite inflammation of the nephritic tubes, as in erysipelas, diphtheria, typhus, pyæmia and pneumonia, in addition to scarlet fever and measles; and that turpentine, cantharides, and arsenic may induce the same mischief.

It appears to me that it is not solely the acrid renal secretion in the exanthemata which induces tubal nephritis. This may partially induce the disorder; but in addition thereto the kidneys may be congested by the additional or vicarious duties imposed upon them; and subsequent to scarlet fever, especially if there has been considerable desquamation of the skin, the liabilities to taking cold are augmented. Always insisting that convalescents from these disorders shall be warmly attired, and avoid too early exposure to cool air, the renal sequel has scarcely fallen under my notice.

In thus observing the etiology of tubal nephritis, the means of prophylaxis are to a certain extent made apparent; but the means, unfortunately, are not always within our reach. The vicissitudes of weather in our latitude, especially during the winter season, occasion many disturbances of the system. The more common are affections of the respiratory organs, but the kidneys are not exempt from their share, and precautions suited to avoid the former are frequently also prophylactic of the latter.

There are also various occupations of life which render those engaged in them liable to abrupt transitions from heat to cold. Those engaged in these vocations are prone to either the acute or chronic form of Bright's disease. Is it not as obligatory to warn such persons of the danger they incur, as to guard either those who manufacture, who mechanically employ, or who are liable to use, materials inimical to health?

The sanitary measures now employed to prevent the spread of the exanthemata and other septicæmic affections will materially diminish the frequency of the disease. Few prodromata; few sequelæ.

Alcohol will doubtless continue to excite the malady; but the other articles mentioned as occasioning renal irritation are seldom used without medical advice, and these will not be rashly prescribed, it being our aim to heal *cito, tuto, et jucunde.*

Desirable as it is to understand the etiology and prophylaxis of acute Bright's disease, a remediable affection, much more important is it to recognize these points in reference to the *chronic* form of the malady, for which no radical cure can be anticipated.

To these topics attention is now invited.

It is not difficult, after Bright's disease is fully developed, to detect an alteration in the composition of the blood, and to trace lesions to such a morbid condition of the circulating fluid. We are less competent to discover the occult influences which are primarily operative in disturbing the blood to such a degree that it can induce the renal disease under consideration. Such influences work slowly and insidiously; the patient is scarcely aware of being unwell; but, unfortunately, during this time abnormal action is excited in the kidneys. Bright's disease is doubtless often attributable to peculiar conditions of the blood, of the precise nature of which we are to a great extent ignorant.

Rokitansky, Walshe, Simon, Johnson, Watson, and others have believed in the humoral origin of the disease—without being able, however, to demonstrate the cause of the change in the blood. There are certain diatheses which we *recognize* as favoring the development of Bright's disease; again, there are causes which may occasion the disorder almost mechanically.

While in many cases our knowledge of the causation is obscure, nevertheless, in many instances the etiology is comparatively clear.

In the work of Dickenson to which we have alluded, the following conditions are enumerated as those to which granular degeneration of the kidneys can be traced:—

"1st. Those conditions which produce and maintain venous congestion of the kidney, such as valvular disease of the heart and pregnancy.

"2d. The gouty habit, from whatever circumstance it arise, but more especially when it is associated with lead.

"3d. A general tendency to fibroid degeneration, as shown by changes in the liver, lung, and other organs."

The depurative or amyloid form of the disease he ascribes to a general change, including the kidneys in common with other organs, and involving the minute blood-vessels, though not ultimately limited to them. "The kidneys," he says, "like the other glandular organs, become infiltrated with a glassy material, which is poured out of the minute arteries, and spreads itself throughout the whole tissue." . . . "The infiltration of this peculiar material occurs especially when the system has been exhausted by a long-continued discharge of pus; and hence the term *depurative* has been proposed as a substitute for the unsatisfactory terms hitherto in use."

In rather more than one-third of the two hundred and sixty-eight cases of these forms of Bright's disease which have occurred in the New York Hospital, causes have been assigned. These, as we have before given them, were: intemperance, fifty-eight; exposures to inclemencies of weather, twenty-three; malarial poisoning, seven; workers in lead, six; opium eating, one; while a number had suppurating sores. Many cases in which the habits were not enumerated, and also in those in which causes were not assigned, we may naturally infer them from the occupations of the individuals. In many cases joint causes may have operated unfavorably upon them.

Dickenson has endeavored to show that alcoholic drinks are comparatively inoperative in causing the disease under consideration. He cites autopsies of fifty-two cases of delirium tremens, in which the kidneys were congested in fourteen, and otherwise diseased, ten; and compares with these fifty cases dying from accidents, in which the kidneys were congested in five, and otherwise altered, fifteen. Now without the history of these latter cases, we must be in doubt whether or not the mishaps occurred to intemperate persons. Again, he compares the proportion of deaths from alcohol with that from renal disease in various places in Great Britain, and as the latter is not as great as the former, thus strengthens his argument.

The reports of the Registrar-General are scarcely precise

enough in their pathological details to establish a point of such importance. If autopsies had been made in the numerous cases of death from alcohol, and the kidneys had been observed by Dickenson's own cultivated eye, renal lesions would have been recognized which less competent observers would have failed to detect. If alcohol is to be excluded as a cause, climate must also be omitted, for while many are exposed to the vicissitudes of the weather, comparatively few have Bright's disease.

The reports of our Metropolitan Board of Health tell a different story from the records of the Registrar-General and of the various cities of Great Britain. In this cosmopolitan city intemperance is very prevalent. One death in two hundred and eleven occurs from alcoholism; while in London the ratio is one in three hundred and seventeen; in Lancashire, one in three hundred and forty-five; in Glasgow, one in four hundred and forty-five, &c. London and New York differ but one degree of Fahrenheit in their annual mean temperatures, the former being 50° F., the latter 51° F. In London one death in eighty-nine occurs from Bright's disease; in New York, one in fifty-two.

Is this disparity in mortality to be ascribed to the greater prevalence of intemperance among our inhabitants? It would seem to be so from the figures which have been given; yet from English statistics it appears that the prevalence of intemperance "has no relation to the frequency of renal disease." We should bear in mind the difficulty of drawing accurate conclusions from mortuary records. This difficulty, to a certain extent, might be obviated if the medical profession would employ a uniform nosology.

Alcohol may suddenly induce death, or may excite acute or chronic diseases. Deaths should be recorded under the diseases which occasioned them; not, ordinarily, under the causes of disease. Cold, intemperance, malaria, &c., are causes of disease. With what propriety could we place "cold" in our nosology, and record under it all deaths occurring from the acute or chronic disorders induced by exposures to atmospheric inclemencies? Many die from distempers caused by intemperance; their deaths are properly inscribed under the maladies which were the immediate cause of demise, and their memories are thus not stigmatized in the public archives.

Many deaths appear under the head of "intemperance" which should be classified elsewhere. The term intemperance may include chronic alcoholism. This latter implies a general condition of disease. Clymer and Anstie employ it to designate the effects of the prolonged use of alcohol upon the human system. These effects are characterized by "permanent congestions in the blood-vessels, hyperplasia of the connective tissue of the organs, and granulo-fatty degeneration."

It is in vain to seek positive information, on certain points, from mortuary reports such as have been enumerated. Statistics derived from such sources may be both interesting and instructive; but in order to settle a doubtful question, their saliencies before adoption must be corroborated by the experience and by the philosophical reasoning of sound medical observers.

The English records cannot be quoted to prove the non-relationship of alcohol and Bright's disease, for our own statistics could be employed to show their alliance. Registrations, both in England and America, are defective, and in deciding the point at issue we must be guided by mature judgment and not by figures, unless they approximate to mathematical precision. Absolute precision will probably never be attained until towards the dawn of the millennium, when physicians will be more accurate in their diagnoses and nearer perfection in their characters.

The experiments of Lallemand, Duroy, Perrin, Anstie, and others have shown that alcohol is partly cast off from the system, unchanged, through the kidneys. When taken to excess the renal emunctories are irritated and congested, and tubal nephritis is occasionally excited by the unwonted stimulus. Those who daily or pretty constantly use intoxicating liquors are, consequently, frequently disturbing the equilibrium of the circulation in their kidneys. The congestions of these viscera may, to a certain extent, be intermittent; but the hyperæmic condition may be so frequent as to border on the persistent. The liver may receive the greater shock from the poison immediately after the alcohol has been absorbed, but the blood is not harmless when it reaches the renal capillaries.

We may even go a step further, and state which liquors directly and which indirectly induce the renal disease under consideration. The distilled liquors, by reason of their containing a

large percentage of alcohol, may occasion the malady simply by their over-stimulating the kidneys—the wines and malt liquors may behave in the same manner, though they are less likely to do so, being more disposed to induce the gout, which disorder, in turn, as we shall hereafter see, is a cause of the Brightian lesion in the kidney. That the malt liquors may excite the gout is a well established fact. Garrod informs us that "among the laboring classes of London gout is very frequently met with; whereas among the same class in Edinburgh and Glasgow it is scarcely ever seen; the former partake largely of porter and beer, the latter almost entirely of whiskey, and in no inconsiderable quantity. The same fact is illustrated in the almost entire absence of gout in many cities on the continent, especially in Poland and Russia, where a distilled spirit is almost exclusively made use of." *

For the reasons that have been enumerated, it appears to me almost conclusive that the continued use of alcoholic stimulants has a tendency to induce Bright's disease, especially so when this cause is co-operative with others which are yet to be mentioned, and which are less readily avoided.

In twenty-three of the cases of chronic Bright's disease occurring in the New York Hospital, the causes assigned were exposure to the inclemencies of the weather. While tubal nephritis may be excited by a sudden change in the weather, granular degeneration of the kidney may be engendered by the often-recurring chills to which the inhabitants of temperate regions are exposed.

Intermittent and repeated congestions from cold induce, as in the case of drinking, a pretty constant hyperæmia of the kidneys. It is probable that the same cause was efficient in many of the other cases, for the occupations of many of the patients necessarily exposed them to harsh atmospheric conditions.

Nor need we be very seriously in doubt whether or not climate is an element in the causation of Bright's disease; I refer to the form of the malady represented by granular degeneration. The *depurative* variety could of course be developed in any section, for suppurating sores may occur in any situation. Careful observers have exhibited the fact that Bright's disease is a malady

* System of Medicine: Reynolds. Article Gout, p. 836, vol. i. London, 1866.

chiefly confined to a portion of the earth's surface. It has been shown that in that belt of the earth, especially has it been observed in the northern hemisphere, in which the revolving seasons are most marked; there the disease is most prevalent.

The inhabitants of the poles and of the tropics are comparatively exempt from the malady, owing to the fact that the climates to which they are exposed are either uniformly cold or equably warm. Other maladies there may be peculiar to polar or equatorial situations, but the kidneys are less liable to the disease we are considering. Other causes may of course operate at such points, as elsewhere; but the climatic influences do not favor renal lesions.

The researches of Dickenson have shown that places "liable to a large proportion of renal disease have an annual mean temperature which is not lower than 46° nor higher than 57°;" and that Bright's disease "chiefly abounds where the mean temperature of the year is not far removed from 50°." He recommends patients to seek a warm and constant climate, and cites the Cape of Good Hope and the coast of the Mediterranean as suitable places of resort during the winter months.

The annual mean temperature of the city of New York and vicinity is 51° F., and therefore it is a location especially favoring the development of Bright's disease. It is consequently not surprising that here the malady is so frequently encountered.

During the year 1867 the deaths in the city of New York numbered 23,441, of which number 425 were from Bright's disease. This malady ranked fourteenth in the order of mortality. It is probable that more died from the disease than are enumerated, the deaths being recorded under the head of uræmia and of various acute or chronic complications. If this number be taken as an accurate one, 1 death in 55 occurred from Bright's disease. In 1866 (nine months) the proportion was 1 in 66. During this year it will be remembered that the cholera was prevalent. In 1868 the proportion was 1 in 42 deaths; the average for the two years and nine months being one death out of each fifty-two.

The following table may illustrate these facts more clearly. The total deaths, and deaths from nephritis and nephria, have been derived from the reports of the Metropolitan Board of Health since the establishment of the Board in 1866.

Year.	Total Deaths.	Ratio of Total Deaths to Population.	Deaths by Acute and Chronic Bright's disease.	Ratio of Deaths by Bright's disease to Total Deaths.	Ratio of Deaths by Bright's disease to Population.
1866*	21,206	1 to 47	323	1 to 66	1 to 3,096
1867	23,443	1 to 43	425	to 551	1 to 2,353
1868	24,889	1 to 40	594	1 to 42	1 to 1,684
Average two years and nine months.	23,179	1 to 43	447	1 to 52	1 to 2,237

* Nine months. Cholera season.

Comparing this mortality with that elsewhere, we find in

London,	1 death in	89—	Mean An.	Temp.	50°.3
Edinburgh,	1 "	95	"	"	47°.2
Glasgow,	1 "	142	"	"	46°.9
Paris,	1 "	266	"	"	52°.0
Bombay,	1 "	2800	"	"	80°.6

If the population of the city of New York is estimated at 1,000,000, we find that one person in every 2237 perished from Bright's disease. On making comparison of this ratio with that observed at English military posts, we find that at

Aldershot—	An.	Temp.	49°.4	1 in 10,059
Gibraltar,	"	"	64°.2	1 " 11,042
Halifax,	"	"	43°.8	1 " 11,667
Bombay,	"	"	80°.6	1 " 12,795

Objection may be made to a comparison of mortality between soldiers and the inhabitants of a city, but, making due allowance for such disparity, we find New York presents an unenviable death-rate for this disease. It is noticed that in comparing soldiers with soldiers, the equably warm Bombay and cool Halifax are in contrast with Aldershot.

Hoping to glean from United States army statistics facts relating to the prevalence of Bright's disease in different sections of this country, I have taken occasion to consult several volumes which officially emanated from the Surgeon-General's office from the year 1839 to 1860.

The terms Bright's disease and albuminuria do not appear in the army nosology. From the nomenclature adopted in the

classification of the diseases, it is to be inferred that the malady under consideration appears under two distinct titles, viz., nephritis and anasarca. From a consolidated abstract of the principal diseases and causes of death occurring among United States troops during the time indicated, excluding the statistics of 1847 and 1848 (war with Mexico), the following facts have been elicited.*

The aggregate strength of the army was 187,144. Number of deaths, 4,910. The number of cases of nephritis was 165, or 1 case to every 1,134 soldiers; the number of deaths from the same cause was 8, or 1 in every 614 deaths. Of anasarca there were 298 cases, or 1 case to every 628 soldiers, and the deaths from this cause were 21, or 1 death in every 234 of the total mortality. It should be stated in this connection that during the same period there occurred 2,372 cases of delirium tremens, or 1 case to every 79 soldiers, and of this number 151 cases terminated fatally, in other words, 1 death in every 33 of the general mortality was from this cause. Of ebrietas there were 6,272 cases, 1 case to 30 of the aggregate strength; and of deaths 67, or 1 in every 73 of the total deaths.

These facts may be tabulated as follows:

Years of observation, 1839 to 1860; aggregate strength of army, 187,144; total deaths, 4,910.

Disease.	Number of Cases.	Ratio to Aggregate Strength.	Deaths.	Ratio to Total Mortality.
Nephritis	165	1 to 1,134	8	1 to 614
Anasarca	298	1 to 628	21	1 to 234
Ebrietas	6,272	1 to 30	67	1 to 73
Delirium tremens	2,372	1 to 79	151	1 to 33

In turning from the consolidated table to the published reports of disease and mortality occurring in the ten military geographical divisions of the United States, including 154 posts, it is unfortunately found that nephritis is classed with calculus, cystitis, diabetes, enuresis, ischuria, orchitis, etc., and is so grouped that

* Statistical Report on the Sickness and Mortality in the Army of the United States. Washington, 1860.

the totals given include all these maladies, and consequently it cannot be learned whether nephritis *per se* is more rife in one geographical division than in another.

Anasarca is similarly grouped with hydrarthrus, hydrocele, and hydrothorax, and consequently cannot be singled out for special investigation.

Even had the frequency of nephritis and anasarca in the different sections been given, it would still have been difficult to decide how far climate had influenced the causation. If intemperance is as rife among our soldiery as the figures given indicate, the disastrous effects of intoxicating liquors in inducing disease can scarcely be over-estimated. Alcoholism stands eighth in the order of mortality, its predecessors in their order being cholera, diarrhœa, phthisis, chronic dysentery, yellow fever, remittent fever, and pneumonia. These data, it will be remembered, extend over a period of nearly twenty years.

Thinking that possibly publications of more recent date than those in my possession might throw light upon the subject, I addressed a note to Surgeon-General J. K. Barnes, U.S.A., informing him of the point of my investigation, and seeking any data that could elucidate the climatology of Bright's disease. In his response he says: "I regret that it will be impossible to supply the information you desire, as no additional statistics of the United States army have been published since 1860. Those to be included in the medical history of the war will refer more particularly to troops in active service and the diseases incident thereto."

Interesting and invaluable as are the extended statistics to which we have referred, and which were compiled and arranged under the supervision of the late accomplished and lamented Richard H. Coolidge, M.D., Surgeon U.S.A., they throw no light upon the topic under consideration.

By the courtesy of Dr. Elisha Harris, Sanitary Superintendent of this city, I have been enabled to examine the mortuary reports of several cities in this country lying north and south of New York. From these reports, and from other works relating to meteorology, and from which the annual mean temperatures in several instances have been obtained, I have drawn such information as has enabled me to prepare the following table:—

Locality.	Annual Mean Temperature.	Length of Observation.	Ratio of Total Deaths to Population	Rat. of Deaths by Bright's Disease to Total Deaths.	Ratio of Deaths by Bright's Disease to Population.
Rochester	46° F.	1 year	1 to 59	1 to 73	1 to 4,286
Providence.....	49° F.	1 year	1 to 54	1 to 173	1 to 9,267
Boston.....	49° F.	1 year	1 to 42	1 to 93	1 to 3,925
New York.....	51° F.	2 yrs. 9 mos.	1 to 43	1 to 52	1 to 2,237
Philadelphia....	54° F.	2 years	1 to 48	1 to 206	1 to 9,934
San Francisco..	55° F.	2 years	1 to 67	1 to 132	1 to 8,827
Charleston.....	67° F.	2 years	1 to 18	1 to 427	1 to 7,778
New Orleans...	70° F.	1 yr. 7 mos.	1 to 28	1 to 329	1 to 9,167

While the figures here presented have been accurately deduced from the sources before enumerated, I cannot confidently present them as being perfectly reliable. The observations have not extended over periods of time sufficiently long to carry that weight of evidence which such statistics are expected to convey. The mortuary reports of cities in the South, for several years past, cannot be consulted with decided benefit to illustrate the point under consideration, owing to the unusual condition of the Southern inhabitants during the recent war. Again, the nosological arrangements are generally so imperfect as almost to preclude exact inferences from them; and further, many individuals perished in New Orleans from yellow fever and cholera who doubtless otherwise would have fallen victims to renal dropsy.

In viewing the table, however, several points are worthy of special notice. Rochester and Providence, with climates cooler than that of New York, have a considerably less mortality from Bright's disease. This comparative immunity may be partially attributed to the fact that these places are nearer the outskirts of the Brightian tract, and partially to the reason that intemperance is less prevalent in those cities than in this locality. For example, Rochester reports no deaths from intemperance or delirium tremens; Providence but two; while Boston, with the same annual mean temperature as the latter place, reports twenty-nine deaths from these causes, and has a very marked increase in the mortality from Bright's disease. Again, in Boston the rate of mortality from intemperance is less than in New York, and there is a less mortality from renal disease,

while the annual mean temperatures of these cities are nearly the same.

The reports from Philadelphia are defective in the classification of diseases. Deaths from Bright's disease are probably recorded under the head of dropsy, and in one year cholera was prevalent, and thus has prevented accuracy in calculating the ordinary ratio of mortality from albuminuria. In Charleston the large ratio of general mortality is attributable to the extreme prevalence of disease among the negroes. In the official report for 1866 it is stated, that in comparison with the year before "the ratio of deaths among the colored population is largely diminished, and this improvement can only be attributed to the thorough and efficient system of hospital and dispensary provisions."

Imperfect as are the statistics which I have been enabled to gather and present, the data are, I think, sufficiently accurate to illustrate the probable truth of the proposition before expressed, namely, that the liability to Bright's disease is diminished where the vicissitudes of the weather are less abrupt than in this locality; in other words, that the climatic element of causation decreases both in more northern and in more southern latitudes.

Fortunately, the territory of the United States is so extensive that the invalid from this section may expect to find within his own country a suitable place of refuge. Our northern frontier pierces the cool north, the southern runs through the warm south,—barely avoiding torridean heat and scarcely escaping torridean miasmata. Intermediate between these boundaries are localities varying as much in annual mean degrees of temperature as in degrees of latitude. Accordingly, east and west through this vast area, from the Atlantic border to the Pacific coast, may be drawn isothermal lines. An isothermal line of 40° F. runs along the eastern portion of the northern boundary, corresponding with 48° of latitude, while on the Pacific coast in the same latitude a temperature of 52° F. is marked. Near our southern boundary corresponding with 27° of latitude an isothermal line of 75° F. may be drawn.

Thirty-five varieties of climate are therefore to be found within our own domain. If the Brightian range lies chiefly between

the mean annual temperatures of 46° F. and 57° F., there are localities north and south of these isothermal lines in this country suited for patients seeking a proper climate.

While various malignant diseases prevail in the tropics and in regions bordering thereon, it need not necessarily happen that an individual in attempting to escape the Charybdis of Bright's disease must encounter the Scylla of an equatorial malady. Though torridean malaria are carried from their birthplace and poison other localities, nevertheless there are districts on this continent, tempered in climate by their proximity to the tropic of Cancer, which are comparatively secluded and exempt from such infection. Such localities possess climates intermediate in character between those which are distinctive of the temperate and torrid zones, and are for the most part free from diseases peculiar to more central zonar situations.

Experience in the New York Hospital, as well as elsewhere, has taught that in many cases of Bright's disease the hot air or vapor bath, conjoined with the internal administration of the spiritus mindereri and ipecacuanha, afford most efficient means of relief.* By their agency diaphoresis as well as diuresis is promoted. I am inclined to think that the cases which can be favorably treated by the method just indicated are improved in this manner. First, increased action of the skin relieves the anasarca, and, to a certain extent, vicariously depurates the blood. Second, the derivation of the blood to the periphery of the body, necessarily induced in favoring cutaneous transpiration, relieves centric and consequently renal hyperæmia: the kidneys thus relieved can, to a limited degree at least, resume their normal function; we mark a decided improvement in the quantity and quality of the urine.

Taking this clinical fact as an analogue, and weighing the inferences drawn from the statistics which have been given, is it not proper to conclude that an individual in removing from here to a warmer climate can diminish a tendency to that nephritic disorder, the etiology of which we are this evening considering?

The southern part of our Union, therefore, seems to present a place of refuge to one threatened with Bright's disease; or, to one

* "Cases of albuminuria treated at the New York Hospital during the three years ending November 1, 1856."—(*New York Journal of Medicine, Jan.* 1857.)

who has recovered from its more serious symptoms, appears to offer a residence in which he is less exposed to excite irritation in parts of the kidneys which may be unaffected, or which may be but partially diseased. In following the isothermal line of 60° F. we find it commencing near the northern part of North Carolina, running through Chapel Hill and Raleigh, thence along the northern part of Georgia, Alabama, Mississippi, Arkansas, and Texas; thence crossing the continent and running northward on the Pacific coast, north of Sacramento, to about the 40° of latitude. Many localities along this route are to be found with good water, a fertile soil, and a freedom from malaria. If on the isothermal line we have selected it should be found that the climate is still too cool, other points on more southern lines are almost as accessible, and their adaptability should be tested.

In thus far considering the climatology of Bright's disease I have spoken of the geographical situations of places in which the malady is most prevalent, in order to contrast them with localities where it is less common, and I have done so for a twofold purpose: 1st, To show the influence of temperature in causing the disorder; and, 2d, To show that convalescents from renal dropsy can, by a change of residence, be placed under favorable conditions to avoid relapses.

We should bear in mind, however, that many such valetudinarians are not in circumstances, pecuniary or otherwise, to avail themselves of the advantages to be derived from a removal from here; and, again, that our entire community is exposed to a climatic cause of nephritic disease. It is our duty to inform the public of the danger that is here incurred, for attention to private hygiene can materially modify unfavorable meteorological influences.

It has been stated that the annual mean temperature of the city of New York is 51° F., and that the abrupt transitions of our weather, especially those occurring during the winter season, favor the development of pulmonary and renal disorders. We cannot expect that either in our own day or in the course of centuries there will be any material change in our climate.

In 1834, Arago, in writing on the Thermometrical State of the Globe, stated that the general temperature of the mass of the earth had not varied a tenth of a degree in 2000 years, but re-

marked: "The surface has become cold in the course of ages, so as scarcely to preserve any trace of its primitive temperature." Again he says, "The changes we have observed, or think we observe, in certain climates, are not connected with cosmical causes, but with circumstances entirely local; such as the clearing of woods and mountains, the drying of morasses, extensive agricultural works, &c., &c. Thus, on comparing the thermometric observations made at Florence, according to the instructions of the Academy of Cimento, towards the close of the 16th century, with those comprised between 1820 and 1830, it was found that the mean remained sensibly the same. It would merely appear that *the winters are not quite so cold*, and the *summers not quite so hot;* a result probably due to the clearings that had been made since this epoch. In the United States, an analogous effect has been observed, in consequence of the vast clearings of which this country is the theatre." *

The late Dr. Joseph M. Smith, in a report on the "Medical Topography and Epidemics of the State of New York," † prepared by request of the American Medical Association, assigned a section of his work to meteorology, and illustrated the subject with barometrical, thermometrical, and hygrometrical tables, and with a map exhibiting the climate of the State. Towards the close of the section alluded to, when treating on the question of whether the climate of the State of New York has become milder or severer since its settlement, he remarked: "The result of the inquiry is, that, in its average character, the climate is the same now, or nearly so, that it was at the time of the discovery of the country; and that the only changes which have occurred in the process of time, if there are any worthy of note, are the following: 1st. A more equal distribution of temperature over the State, and throughout the year. 2d. In those parts in which the country has been uncovered of its forests and brought under cultivation, the mildness of the autumn has been extended into the winter; and, 3d. The severity of the winter, owing to the deeper congelation of the earth, has encroached upon the spring. The severe winters which occur at irregular intervals are exceptions to the general fact, that the climate has undergone no

* Kæmtz's Meteorology. By C. Walker. London, 1845, p. 153.

† Transactions of the American Medical Association, vol. xiii., 1860, pp. 81 to 269.

material change within its historic period. It is said by Prof. Dove, 'that the different degrees of severity in the winters of different years depend so exclusively on the prevailing direction of the wind, in each case, that there can be no doubt as to the more immediate or approximate cause of this severity.' "

Judging from the past, we are justified in supposing that the climate of the city of New York is not to become appreciably modified in its nature, and that in the future the inhabitants will suffer from the same climatic disorders as are now experienced.

It behooves *all*, therefore, who reside in this metropolis during the winter months to maintain a constantly uniform and normal temperature; an object which can only be attained by suitable diet, warm clothing, and a due attention to the warming of apartments. Upon those who are recently convalescent from renal dropsy, this injunction is imperatively binding.

There are causes of Bright's disease which to a certain extent are hereditary. There is not, properly speaking, an hereditary Brightian diathesis; but transmitted diatheses which favor its development. I refer more particularly to the rheumatic and gouty, which may be the occasion of granular kidney, and to the cancerous, syphilitic, and tubercular, which by in time inducing profuse suppuration, can in turn occasion the "amyloid" kidney.

Rheumatism and gout are frequent causes of disease of the heart. Cardiac affections lead to chronic congestion of the kidneys, and subsequently to granular degeneration, and in addition thereto in gout there is a special lesion of the kidney; viz.: a deposit of urate of soda between the tubules, exciting alteration in structure, and necessarily defective function—there is, in a word, a gouty Brightian kidney. While we are aware that rheumatism and gout may occur in those not predisposed to them, nevertheless the lactic and uric acid diatheses are more readily developed in those having a tendency to them.

In the 78 autopsies before alluded to, 33 presented instances of hypertrophy of the heart, and 18 of valvular disease. During the course of Bright's disease, pericarditis and endocarditis are not unfrequently excited by the toxæmia; these, if the patient survive, may lead to hypertrophy and valvular disease of the heart, augmenting the gravity of the first disease. The cardiac affection, however, is frequently the antecedent one, and there-

fore may be the legitimate cause of the renal malady. There is, therefore, as Dickenson has expressed it, a "double relationship" between the disorders. "The kidneys are granular in more than one-third the cases of valvular disease."

The concurrence of the uric-acid diathesis and albuminuria was recently observed in a patient in the New York Hospital. He was laboring under a gouty affection of one foot, and had suffered from several similar attacks. No symptoms indicative of renal disease were present. By my request Dr. Lucius D. Bulkley, Resident Physician, examined the urine chemically and microscopically. He reported the reaction acid and the presence of albumen, of pus, and of blood in small quantities. No casts were found. The albumen may have been entirely ascribable to the pus and blood. The patient, at the time of writing, is still under observation. He is employed as a nurse in the institution, and, in addition to our present investigation, he can be examined from time to time for nephritic disease after his recovery from the acute malady under which he is suffering.

The painful local affections, the cardiac lesions, and renal alterations present a threefold necessity for avoiding the development of the lactic and uric acid diatheses.

Magendie has recorded one of the most extraordinary instances illustrative of the production and prophylaxis of the uric acid diathesis. "Mr. H., a merchant in one of the Hanseatic cities, possessed in 1814 an ample fortune, and he lived in accordance with his means—kept a good table and indulged in its pleasures freely. He was at that time tormented with gout and gravel. Unexpectedly he lost all his fortune through a political crisis, and was obliged to take refuge in England, where he lived more than a year almost in poverty, amid numerous privations; but his gravel completely disappeared. Little by little he succeeded in repairing his affairs; he resumed his old mode of life, and the gravel was not long in reappearing. A second reverse robbed him in a short time of all he had gained. He passed into France almost without resources, and his regimen was consonant to his means; the gravel disappeared. Once again his industry restored him to a life of plenty and ease; he abandoned himself again to the indulgences of the table, and with them appeared once more his

old enemy, the gravel."* Similar observations must have been made long antecedent to the time of this occurrence, and yet we read that in 1739 the English Parliament assigned a reward of £5,000 to Miss Joanna Stephens for the secret of the composition of a nostrum which she possessed as a remedy for the disease.†

Modern pathology, however, reveals lesions primarily induced by errors in diet, which were not distinctly recognized by our medical predecessors. The gouty form of Bright's kidney is one of these.

It is an old saying that there is nothing new under the sun. Had Milton some glimmering knowledge of our present etiology when, in his Paradise Lost, he makes the archangel Michael, in addressing Adam, say:

> "By intemperance, more
> In meats and drinks, which on the earth shall bring
> Diseases dire: of which a monstrous crew
> Before thee shall appear, that thou may'st know
> What misery the inabstinence of Eve
> Shall bring on men"?

And again, how strikingly does he illustrate the prophylaxis, when the archangel, in addressing Adam, says:

> "If thou well observe
> The rule, of not too much, by temp'rance taught,
> In what thou eat'st and drink'st, seek from thence
> Due nourishment, not gluttonous delight,
> Till many years over thy head return:
> So may'st thou live, till, like ripe fruit, thou drop
> Into thy Mother's lap, or be with ease
> Gathered, not harshly pluck'd for death mature."

Milton died in 1674, and, singular to say, of the disease we have just been considering, viz., of the gout, in the 67th year of his age. His biographer informs us that "in his diet he was abstemious; not delicate in his choice of dishes; and strong liquors of all kinds were his aversion." The fact of his attaining such an advanced period of life leads us to suppose that he put in practice his ripe views of prophylaxis.

* Urinary and Renal Diseases. William Roberts, M.D., Manchester. Am. Edit., 1866, p. 235.

† Ibid., p. 237.

There are other exciting causes of Bright's disease to which I have not alluded. I refer especially to pregnancy and certain occupations. In pregnancy congestion of the kidneys has been ascribed either, on the one hand, to pressure upon the emulgent veins, or on the other, to a reflex nervous influence, as suggested by Dr. A. Clark. If the congestion occurs towards the close of utero-gestation, the hope is to be entertained that on the expulsion of the fœtus the kidneys will resume their normal condition; if it occurs early, permanent injury may take place in the renal texture.

Avoiding a consideration of the treatment of such cases, I would simply remark, that in cases in which Bright's disease is manifested early during pregnancy, the obstetrician may have to decide the question whether or not delivery shall be expedited, not simply to relieve immediate disaster to the patient, but to remove a condition which, if continued, would eventually lead to the death of the mother; at a time, perhaps, long after the period of natural gestation.

The fact is now recognized that certain occupations of life are conducive to Bright's disease, those individuals whose employments bring them in contact with the various saturnine preparations seeming to have a tendency to the disease.

About twenty years since appeared the work of Tanquerel on lead diseases. The volume at the time seemed to give an exhaustive exposition of the subject on which it treated, the author founding his views on an observation of over two thousand cases occurring in the Charity Hospital, Paris. Shortly following came Melsens with his brilliant discovery, throwing a flood of light on the prevention and treatment of these disorders, by his physiological and therapeutical experiments on the inferior animals with the iodide of potassium. No allusion, so far as I am aware, was made in these treatises to any saturnine renal malady.

The credit of calling attention to the relationship between lead poisoning and the uric acid diathesis appears to be due to Dr. Alfred Baring Garrod. He says: "About fifteen years ago, I was struck with the fact that a large percentage of the gouty patients which had come under my care in hospital practice consisted of painters, plumbers, or other workers in lead, and a more careful investigation of the subject forced the conclusion on my mind,

that the influence of this form of metallic impregnation in inducing a gouty condition of the system was very considerable." * * * "It may be considered as established that the metal lead acts as a powerful predisposing cause of gout." * * * "In some instances, severe attacks of gout have been induced in gouty patients by the medicinal administration of lead salts, on account of the occurrence of epistaxis or other forms of hemorrhage; and the frequent recurrence of the seizures whenever the medicine has been repeated has satisfactorily shown that the phenomena were to each other in the relation of cause and effect."

"On investigating the physiological action of the administration of lead salts, it is discovered that a very decided effect is produced by that metal upon the secreting power of the kidney, as far as uric acid is concerned; the function is notably diminished." * * "The blood of individuals suffering from lead paralysis always contains an abnormal amount of uric acid; and the same probably holds good in all cases of lead colic." *

To Ollivier, in 1863, is assigned the honor of demonstrating the production of Bright's disease by saturnine impregnation. Melsens, to whom we have alluded, experimented on the inferior animals to learn the antagonism between the iodide of potassium and the lead preparations. Ollivier induced lead poisoning in dogs, rabbits, and guinea pigs, to learn whether the kidneys were affected thereby, and ascertained that he could produce "incipient organic disease." His views were confirmed by the observation of cases of lead diseases coming under his treatment in the Charity Hospital, Paris, and have been endorsed by Lancereaux and Danjoy.

It thus appears that the metal lead exerts an extraordinarily unfavorable influence upon the human economy. In the first place it may manifest its baneful effects by colic, neuralgia, paralysis, etc.; in the second place, by inducing the gouty diathesis, it favors depositions of urate of soda, and such deposits, when present in the kidneys, excite therein structural alteration; and thirdly, the experiments of Ollivier have shown that the presence of the metal in the renal texture is calculated to produce a similar lesion. It seems that patients may manifest either the uric acid

* Reynolds' System of Medicine. London, 1866. Vol. I., pp. 841, 842.

diathesis or the nephritic malady without having previously encountered the ordinary disorders indicative of saturnine poisoning.

Six of the patients suffering with Bright's disease in the New York Hospital were workers in lead. In these days, when the metal is so extensively employed for various purposes, many are accidentally exposed to its influence, not workers in its manufacture. In how many persons, with perhaps a peculiar susceptibility to its influence, may it induce the gout or even the renal disorder without its agency being at all suspected?

The causes of Bright's disease which have thus far been particularly enumerated are those which induce an hyperæmic condition of the kidneys, and as a result of such congestion there occurs tubal nephritis or intertubular disease, not necessarily, or at least primarily, involving disease in the blood-vessels. There remains to consider another cause, viz.: an abnormal constitution of the blood, depending upon conditions to be hereafter enumerated. The morbid property of the blood excites disease in the renal capillaries, and finally, by an exudation from these diseased vessels, the general structure of the kidneys is so altered that they present the amyloid, waxy, or depurative variety of degeneration. That the morbid phenomena in the kidneys are attributable to a peculiar condition of the circulating fluid is inferred from the fact that the same species of lesion is simultaneously to be observed in other viscera and tissues of the body.

The chemical composition of the exudation just alluded to has been an object of special investigation. Virchow, Schmidt, Kekulé, Dickenson, and others have endeavored to define its character—possibly its nature has been at last discovered. Virchow believed it, from certain of its reactions, to be composed of ternary principles analogous to starch, and hence termed it "amyloid." Later, Schmidt and Kekulé discovered that it contained carbon, hydrogen, oxygen, and nitrogen. and was similar to albumen and fibrine in composition; and finally Dickenson has declared that "the material which is poured out under certain conditions, and which has been styled waxy, amyloid, or depurative, is de-alkalized fibrine." Waxy degeneration, when observed in the kidneys and supra-renal capsules, is generally also found in the liver, spleen, and lymphatic glands. The mucous membrane

of the intestinal canal is likewise impaired, though in a somewhat different manner, on account of a difference in structure.

The question naturally arises, what conditions occasion in the blood such alterations as will favor a deposition of fibrine in the arterial capillaries, and finally throughout the parenchyma of viscera? Rosenstein has gathered statistics relating to 100 cases in which autopsies revealed waxy kidneys, and Dickenson in regard to 51 cases. In each one of these instances the patients had been prostrated by chronic suppuration, having suffered either with tuberculosis or with caries, syphilis, cancer, psoas abscess, abscess of liver, long-continued bed sores, dysentery, ulceration of the bowels, or with chronic alcoholism. My own experience confirms the observations of these gentlemen.

The rationale which has been offered for the occurrence of waxy degeneration as a sequel of purulent discharge is as follows: The blood, losing a large quantity of albumen and of alkaline matter through the drain of suppuration, becomes relatively surcharged with fibrine, which principle in a feebly alkaline fluid is liable to be deposited in the arterial capillaries, leading to disease in the vessels, which finally extends into surrounding tissues.

There are other causes which may occasion a similarly unfavorable condition of the blood. For example, a patient may be suffering with the form of Bright's disease we have before treated of this evening. The albumen which occurs in the urine may make such a drain upon the blood as to induce a sanguineous condition similar to the one just alluded to as occurring during profuse suppuration. Hence a compound cause may be operative in destroying the integrity of the renal tissue, and consequently disease is found in the nephritic tubes, as well as in the intertubular structure and in the blood-vessels. Again, there may be occult influences inducing a similar impoverishment of the blood and leading to the amyloid degeneration. Whether the rationale presented is correct or not, it nevertheless seems evident that there is a relationship between chronic purulent discharge and waxy degeneration, and the connection seems that of cause and effect.

Our ability to prevent this form of Bright's disease depends on our ability to cope with the diseases or accidents which give

rise to it. Surgeons as well as physicians encounter the malady. Many of the affections which occasion profuse suppuration result from inherited diatheses. Surgical operations liable to be followed by long-continued purulent discharge, as, *e. g.*, exsections, amputations, &c., may consequently induce the depurative infiltration of the kidneys and of other viscera.

If suppuration is already taking place, in order to avoid the amyloid sequelæ the blood must be retained as far as possible in its natural condition. To supply the albumen and alkalies lost in the purulent discharge, and to furnish a quantity equal to the demands of assimilation, requires food rich in its possession of the protein compounds, and, in addition thereto, a more liberal amount of alkaline principles than is ordinarily ingested. These latter should not be given with the meals, lest their presence impede the process of digestion.

We are debarred from exhibiting such food when ordinary Bright's disease is occasioning the liability to amyloid degenerations. Such azotized food, when renal disease already exists, would augment the danger of toxæmia, as before shown. I would suggest that in such cases it may be appropriate to exhibit the alkalies, and thus retain the blood in its normal chemical relation to the fibrine—the aliment allowed being only moderately nitrogenous in character.

Before dismissing these topics, one word in regard to the therapeutics of Bright's disease.

One of the sketches by Hans Holbein, the renowned painter, born in 1498, illustrative of the celebrated Dance of Death, relates to our profession and is appropriate to our subject. Beneath it are the words, "*Medice, cura te ipsum.*" It represents an apartment containing an ample library, and in which is seated a physician at his desk, with an hour-glass before him. Death stands beside him, with one hand presenting a patient to the physician, and with the other offering a urinal containing the water of the invalid, and seems to say, "Canst thou cure this man who is already in my power?"

This sarcastic sketch, which has survived the lapse of more than three centuries, our own limners could truthfully imitate. Death might now be depicted as leading a patient with Bright's disease to the modern physician, with his accurate tests and

delicate lenses, and as scornfully asking the same question. Unfortunately, our etiology and pathology are in advance of our therapeutics. Though science has not revealed to us any specific remedy for the confirmed disorder, the means at our command are efficient in its alleviation.

A large proportion of the cases coming under treatment are relieved, and the patients are enabled to resume the ordinary duties of life. We place them in the same condition as are those who have the latent form of the malady, and their hygienic management can be so ordered, especially under favorable climatic influences, that they may expect to escape for a season the more uncomfortable and unfavorable symptoms of their malady. In some instances we apparently afford permanent relief.

In the discussion on Bright's disease which took place in this Academy a few years since, the department of Therapeutics was assigned to a member now deceased.* No advance has been made in our knowledge of this subject since his views were expressed.

The success in the treatment of the disease in the New York Hospital is due in great measure to a remedy (the corrosive sublimate) which, as attending physician, he suggested as a means of relief nearly thirty years ago, and which to this day is resorted to with confidence in certain cases.

But I will no longer dwell on this subject. The disease we have been considering is of such gravity, involving lesions of such important viscera, that we may well fear that science will never reveal to us any radical cure.

Imperfect as has been this *résumé* of the etiology of Bright's disease, it must be apparent that the malady can to a certain extent be avoided by a strict observance of the laws of hygiene. Unsatisfied with our present meagre knowledge of its etiology and prophylaxis, these points are worthy of special investigation, not in the expectation of entirely banishing the disorder from our nosology, but in the hope of diminishing the frequency of its occurrence.

* Dr. Joseph M. Smith, "Therapeutics of Albuminuria." Bulletin of the New York Academy of Medicine, vol. ii., pp. 40–51.

ON THE

PATHOLOGICAL CONDITIONS

CAUSING

STERILITY IN THE FEMALE.

THE Academy at its last meeting having been favored with a dissertation on the theory of "Fecundation, Sterility, and Superfœtation," I have thought it an object not unworthy of the attention of this learned body to treat the question of sterility from a practical point of view.

The paper which I take the liberty to read before you this evening is based chiefly on pathological anatomy, and contains a truthful report of the pathological condition found in the aggregate number of sterile women who have come under my observation during the last ten years, and of which accurate records have been kept either in my books or those of the German Dispensary, to which I have been attached since its foundation, in the year 1858.

Many cases had to be omitted, either because an exact diagnosis was not arrived at, or because the notes taken down were deemed imperfect.

I am aware that this is not the first attempt of this kind made, Dr. Charles Mayer, of Berlin, having, in the year 1856, published a paper in which he enumerates the causes of sterility as found in 272 cases observed by him. Among these he found 2 without uterus, 60 with anteflexions, 37 retroflexions, 35 anteversions, 3 retroversions, 42 cases of vulvitis, and, among these, 14 with an entire hymen after several years of married life, 51 cases of chronic endometritis, 25 cases of oophoritis, 23 ovarian tumors, 12 uterine polypi, 6 fibroid tumors of the uterus, 1 elephantiasis of the external genitals; in 6 cases no pathological conditions could be found.

In the examination of sterile females we may sometimes find one pathological condition only, for instance, catarrh of the

uterine canal; but these cases are the exception and not the rule. Generally we meet with a combination of lesions, which renders it doubtful whether the sterile condition should be attributed more to the one or to the other, as, for instance, where we find adhesions, displacement, and catarrh combined. For a complete understanding of the cause of sterility in a given case, it is indispensable to take into account both the various anatomical lesions which are found to exist in the sexual organs, and the functional derangements connected therewith. This is especially necessary if we wish to calculate with anything like probability in regard to the success to be expected from treatment.

From an early period I have been in the habit, before examining a patient suffering from sterility, to commence by inquiring into the subjective symptoms presented by her, and for the relief of which she came to me seeking for medical advice. Some had nothing to complain of, and merely wished to know whether there was any material cause for their having no children, and whether such cause could be removed. Others seemed to ignore their sterile condition, and simply desired removal of certain functional derangements, chiefly of menstruation, nervous disorders, disturbed digestion, and painful sensations in various parts of the body.

The examination was then proceeded with, and its results carefully noted down under the following heads:

1. Anomalies of Suspension.—To these pertain the various forms of versions, descensus, and other displacements of the uterine body and cervix.

2. Anomalies of Uterine Tissue.—Here are considered hypertrophy and atrophy, contractions of the canal, and adventitious growths originating in the uterine tissue.

3. Uterine Catarrh in its Various Forms.

4. Lesions of Organs in Proximity to the Uterus, chiefly of the Peritoneum and the Appendages.

5. General Conditions and other Anomalies accidentally combined with Lesions of the Uterus.

The total number of cases observed was 408. Of these 201 occurred in private, 207 in clinical practice. The question here occurs, After what lapse of time is it allowable to place a female living in the married condition on the list of sterile women?

This, of course, is arbitrary with the observer; but as a conclusion must be arrived at, after mature reflection I was led to make a distinction between three classes:

1. Such as were married during a certain lapse of time and had never become pregnant. In these a period of two years was deemed sufficient.

2. Such as had been pregnant but had miscarried once or several times. For these, likewise, two years were allowed.

3. Such as had given birth to one or several children, and thereupon remained without further issue for a certain number of years, although remaining in the married condition. Here a minimum of five years was considered necessary to have elapsed.

The result of the computation is as follows:

Females two years married who were never pregnant	140
Such as had been pregnant, but had miscarried,	83
Such as had borne children, and then remained sterile for five years or more	185

In the latter class (numbering 185) sterility had existed:

From 5 to 10 years in	140
From 10 to 15 years in	33
From 15 to 20 years in	11
Over 20 years in	1

Of the whole number of 408, there are noted as sterile:

Under 5 years	147
Between 5 and 10 years	193
Between 10 and 15 years	50
Between 15 and 20 years	16
Over 20 years	2

Subjective Symptoms and Functional Derangements.

The motives which induced our patients to seek for medical advice are set down as follows:

Dysmenorrhœa	69
Menorrhagia and metrorrhagia	57
Scanty menses	41
Premature cessation of menses	4
Never menstruated	2

Retarded menses	8
Habitual miscarriage	3
Hysteria	16
Nervous headache	3
Vaginismus	2
Intercostal neuralgia	1

The number of those who asked for relief of the sterile condition is not noted. The same remark must be made of the many others who complained of painful sensations in the pelvic region, and other ailments standing in remote relation only to the sexual organs. I will now pass to the relation of the anatomical conditions found upon examination in the order above-mentioned:

1. *Anomalies of Position of the Uterus*—

Retroversion	20
Anteversion	18
Dextroversion	10
Sinistroversion	10
Descensus	8
Prolapsus	1
	67

2. *Anomalies of the Uterine Tissue*—

These are distributed as follows:

Anteflexion	83
Retroflexion	71
Hypertrophy of uterus	65
Atrophy of uterus	3
Atrophy of cervix	1
Infantile uterus	2
Small os	24
Stenosis of entire cervical canal	11
Stricture of internal orifice	35
Fibrous tumors in walls of uterus	10
Carcinoma	5
Polypus	6

3. *Catarrh*—

Of the whole number of 408, 342, consequently about seven-eighths of the whole number, were afflicted with uterine catarrh

of some form or other, the secretion varying between the serous, mucous, purulent, or a combination of both the latter.

In the majority the affection seemed to be limited to the cervical canal; in those, however, suffering from flexion of the organ, or stricture of the canal, the dilated cavity of the body was undoubtedly the seat of active hyper-secretion.

4. *Affections of Organs in Proximity to the Uterus*—

The peritoneum and the appendages were frequently found in a diseased condition. Thus, there were:

Cases of acute or subacute perimetritis and peritonitis	12
Firm adhesions resulting from previous attacks of peritonitis	83
Ovarian tumors	14
Peri-uterine tumors, with undefined seat	7
Gonorrhœa	2
Acute colpitis	1
Pelvic abscess	1

5. *General Conditions and Accidental Diseases*—

Among those belonging to this class, and worthy of remark, I will mention:

Secondary syphilis	8
Valvular disease and hypertrophy of heart	5
Tuberculosis	4

No anatomical lesions whatever were found in two cases.

It would be an interesting matter to know the exact proportion of patients in whom a cure of the sterile condition was effectuated by a treatment based upon the anatomical lesions found in every individual case. This, however, in clinical practice, is an impossibility, from the fact that the majority, with the exception perhaps of those suffering from hysteria, are transient visitors at our public institutions, who underrate the value of medical services which cost them no money. Many being wearied of the duration of treatment, or unwilling to comply with the manifold inconveniences connected with it, interrupt treatment a short time after it is commenced; the termination of others is never heard of.

Hence it is not astonishing that in the books of our dispensary no more than 3 cases are recorded as having been cured.

In private practice, although you meet with the same obstacles, the conditions are somewhat more favorable, as in general you have to deal with a more intelligent class of people, who are willing patiently to await the result of their perseverance, and expect to get the full value of the money which they pay for the services of the physician. Of the patients treated in private practice, who are 201 in number, 25 are put down as having afterwards given birth to full-grown children. About 100 of these were either subjected to no treatment, because they presented an unfavorable prognosis, or withdrew from treatment for some reason or other. About 25 are still under treatment at the present day. If we deduct these 125, it will be seen that treatment was successful in 25 out of 75, or about one-third of the cases treated.

I shall now enumerate the whole list of these 25 cases in a short review of the symptoms which they presented, and the treatment pursued in each individual case:

Patients Treated in Private Practice.

No. 1, Mrs. A., aged 24, married three years; never pregnant; suffered from vaginismus, hypertrophy, and retroflexion; became pregnant after about six months' treatment, consisting in dilation of the vagina and Scattergood's pessary, which she wore until pregnancy had advanced to four months. She has since given birth to four children in three years.

No. 2, Mrs. Br., aged 30, married ten years; had one child seven years before entering on treatment; suffered from dysmenorrhœa, hypertrophy, and catarrh; treatment was continued over a year, and consisted in intra-uterine injections, and application of topical remedies to the uterine cavity. She bore one child, which is still alive, and is now a widow.

No. 3, Mrs. Be., aged 31, married ten years; had one child previous to the commencement of treatment, no miscarriages; suffered from hemorrhages, dysmenorrhœa, and catarrh; treated by dilatation of uterine canal and intra-uterine injections. Was delivered of a healthy child about a year after the commencement of the treatment.

No. 4, Mrs. F., aged 30, married eight years; never pregnant; suffered from dysmenorrhœa, scanty menses, anteflexion, catarrh, and stricture of internal orifice; treatment was persevered in for a year and a half, and consisted in dilatation of cavity, and application of the nitrate of silver. Is now the mother of four children.

No. 5, Mrs. Ge., aged 24, married two years; had no children, but two miscarriages; suffered from hemorrhages and retroversion; treatment was continued for several months by replacement of uterus with the sound. Has had several children since.

No. 6, Mrs. Go., aged 25, married five years; no children, two miscarriages; suffered from habitual miscarriages, catarrh, and secondary syphilis. She was under treatment for nearly a year, when she became pregnant, carried to the full term, gave birth to a healthy living child, but died herself from hemorrhage after delivery.

No. 7, Mrs. Hu., aged 22, married four years; no children, one miscarriage; suffered from dysmenorrhœa, retroversion, and catarrh; treated by replacement of uterus with the sound, and intra-uterine injections. Treatment was discontinued after six months, and after a six months' pause she became pregnant, and is now the mother of several children.

No. 8, Mrs. He., aged 22, married two years; never pregnant; suffered from dysmenorrhœa, retroflexion, and catarrh; treatment was continued for a year, and consisted, first, in replacement of the uterus with the sound, and, afterwards, application of Hodge's pessary: no alteration of tissue being visible in the mucous membrane of the canal, and the secretion being of a gelatinous nature, without admixture of pus, it was considered symptomatic of the derangement in circulation, hence no special treatment for catarrh was resorted to. She was delivered two years after the commencement of treatment.

No. 9, Mrs. Me., aged 33, married seven years; one child six years previous to treatment; no miscarriages; suffered from retroflexion. Eighteen months after the commencement of treatment—which consisted in replacement of uterus with the sound, followed by application of Hodge's pessary—she became pregnant, carried to the full term, and bore a living child.

No. 10, Mrs. Mu., aged 30, married three years; never preg-

nant; suffered from catarrh; treated by intra-uterine injections, dilatation of uterine canal, and application of the nitrate of silver. Became pregnant after about six months' treatment, and has since been twice delivered of healthy living children.

No. 11, Mrs. My., aged 31, married seven years; never pregnant; suffered from scanty menses and catarrh; treated during several months by dilatation with large sounds, and application of nitrate of silver to the uterine mucous membrane. She then became pregnant, and has since been delivered of three living children.

No. 12, Mrs. P., aged 25, married four years; never pregnant; this was certainly the most remarkable case of cure which ever occurred under my observation: no alteration of tissue whatever could be discovered, beyond a stricture at the internal orifice; this was dilated in one session, shortly after the menstrual epoch; she was told to return immediately after the next menstruation to have the operation repeated. To her and my astonishment the flow did not return at the time when it was expected; she was pregnant, carried to full term, and has since been delivered of three healthy children.

No. 13, Mrs. R., aged 25, married three years; never pregnant; suffered from vaginismus; treatment consisted in manual dilatation of the vagina, and dilatation of uterine canal with large sounds. Shortly after she became pregnant, and carried to full term.

No. 14, Mrs. Sn., aged 39, married twelve years; four children; last five years previous to treatment suffered from menorrhagia, hypertrophy, and catarrh; treated by dilatation of uterine canal and intra-uterine injections. Her cervical catarrh was treated by applications of nitrate of silver, and treatment was successful after a duration of a few months. She is now under treatment for a fibrous tumor of the uterus.

No. 15, Mrs. Schu., aged 28, married three years; never pregnant; suffered from hypertrophy, catarrh, and small fibrous tumor, distinctly perceptible through the fundus of the vagina and the abdominal integuments; the uterine cavity was dilated with large sounds during about half a year, when she became pregnant, and has since given birth to two full-grown children.

No. 16, Mrs. Sche., aged 28, married eight years; never pregnant; suffered from hypertrophy of cervix and uterine catarrh;

treated by dilatation of canal, and application of nitrate of silver. Became pregnant after a year's treatment, and carried to full term.

No. 17, Mrs. Schi., aged 35, married fifteen years; had two children, the last eight years before treatment; suffered from hysteria, retroversion, and catarrh; treatment, replacement of uterus with a sound, no pessary and intra-uterine injections. She was delivered about a year after the commencement of treatment.

No. 18, Mrs. Sab., aged 23, married three years; no children, two miscarriages; suffered from perimetritis; treated by antiphlogistic treatment and opium. Became pregnant after a few months, and has since been twice delivered.

No. 19, Mrs. Sak., aged 30, married seven years; had one child six years before treatment, followed by eight miscarriages; suffered from anteflexion and catarrh; treatment consisted in dilation of uterine canal, and injections of compound tincture of iodine. After a lapse of about six months she became pregnant, and was delivered of twins at full term.

No. 20, Mrs. Schw., aged 35, married fifteen years; had two children, last thirteen and a half years previous to treatment; suffered from catarrh; treated by dilatation and intra-uterine injection, and is now pregnant eight months.

No. 21, Mrs. St., aged 29, married twelve years; never pregnant; suffered from retroflexion; treated by replacement, followed by application of Hodge's pessary; became pregnant after a short treatment, and was delivered of a healthy living child. The pessary was removed during the fifth month of pregnancy.

No. 22, Mrs. Schwa, aged 27, married five years; no children, two miscarriages; suffered from slight anteflexion and catarrh; treatment consisted in dilatation of uterine canal by large sounds and injections of diluted carbolic acid. After about a year's treatment she became pregnant, and is now expecting her delivery.

No. 23, Mrs. U., aged 35, married four years; no children, one miscarriage; suffered from catarrh and stricture of internal orifice; treated by dilatation with large sounds and intra-uterine injections of compound tincture of iodine. After about three months' treatment she became pregnant, and carried to full term.

No. 24, Mrs. We., aged 30, married six years; never pregnant; suffered from scanty menses, extreme retroflexion, catarrh, and stricture of internal orifice. When she came under treat-

ment she was in a wretched condition, her feebleness scarcely permitting her to walk, reduced in flesh, and exceedingly nervous. At the commencement the uterus was replaced by gradual endeavors with the sound, the catarrh was treated by injections of the comp. tinc. iod.; treatment was continued for five years; during the last year she wore a Scattergood's pessary, which was continued in use until the enlarged fundus began to distend the abdomen. She was happily delivered at full term, and has regained tolerable health.

No. 25, Mrs. Zi., aged 25, married nine years; never pregnant; suffered from dysmenorrhœa, retroflexion, and catarrh; the uterus was replaced by the sound, the uterine canal dilated, and its mucous membrane treated by application of comp. tinc. of iod.; a Hodge's pessary was applied as soon as she was able to bear it, and she is now pregnant about seven months.

From the foregoing statement it appears that the most favorable prognosis for the treatment of the sterile condition is presented by those cases in which sterility is caused by flexion of the uterus, chiefly retroflexion, and catarrh of the uterine canal; not one case, where extensive adhesions were present, or lateral deviations existed, is marked down as cured; neither was I successful in any of those numerous cases where anteflexion was considerable, or the external orifice exceedingly small, which is probably owing to the circumstance that up to a very recent date I had abstained from incising the os, as is now the fashion. It seems to me that in the latter class of cases dilatation by means of the sound, which promises excellent results in cases of stricture of the internal orifice, is insufficient to produce that degree of viability of the cervical canal which I consider as indispensable to the successful treatment of uterine catarrh.

The proportion of cases cured being only 33 per cent., the result of my endeavors has not been very favorable; still it is instructive in many ways, as it shows what can and what cannot be accomplished without the aid of the knife, and teaches us in what cases treatment should be abstained from.

Let us hope that with the aid of the new methods recently introduced into uterine surgery, and other means yet to be discovered, we may be enabled to reach the object in view with less loss of time and greater security.

ERRATA.

Page 22. In the diagram the upper line "o m c" should stand clear of the dotted line.

" 45. Running head—insert "determining" between "Forces" and "Calibre."

" 105. Running head—*for* "Temperature *of* Injury to Nerves," *read* "Temperature *in* Injury to Nerves."

" 119. Running head—*dele* "h" in "Paraplegia."

THE

PHYSIOLOGY AND PATHOLOGY

OF THE

SYMPATHETIC OR GANGLIONIC

NERVOUS SYSTEM.

BY

ROBERT T. EDES, M.D.

AN ESSAY TO WHICH THE O'REILLY PRIZE WAS AWARDED BY THE

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PREFACE.

I WISH especially to disclaim any attempt at historical completeness. I have used such authorities as best suited my purpose, endeavoring, as far as possible, to refer to original papers, though without intending thereby to express any judgment as to priority of discovery. Such a judgment should depend on more abundant material than was at my command, and would greatly add to the time and labor necessary for this essay; an addition, it seems to me, which would be, for the purposes therein had in view, nearly wasted.

I have to regret the small number of facts I have been able to contribute; and I claim, as the only merit of this essay, to have arranged what is known upon the subject into a systematic whole, and to have so brought together isolated observations as to harmonize with, and throw light upon, each other.

In excuse for my theories, I will say, in the words of Paget:—

"While we are ignorant of the nature of some of these forces, it "is better that their places in our minds should be occupied by "reasonable hypotheses, than that they should be left blank, or be "overspread with the tinge of one exaggerated theory."

ROBERT T. EDES.

HINGHAM, February, 1869.

CONTENTS.

PHYSIOLOGY.

SECTION		PAGE
	Anatomy	7
1.	Sensation	14
2.	Experiments on Head and Neck	16
3.	Pupil, and some Smooth Muscles	18
4.	Temperature	28
5.	Circulation	39
6.	Heart	56
7.	Secretion	61
8.	Nutrition	73
9.	Intestines	77
10.	Pelvic Viscera	80
11.	Summary	82

PATHOLOGY.

	Anatomy	84
1.	Neuralgia	86
2.	Iris. Progressive Locomotor Ataxy	88
3.	Temperature	93
4.	Heart	108
5.	Secretion—Diabetes	111
6.	Nutrition — Exophth. Goître — Skin Diseases — Epilepsy — Reflex Paralysis	117
7.	Intestines	150
8.	Uterus	150
9.	Summary	151

PHYSIOLOGY.

ANATOMY.

Before proceeding to the physiology proper of the great sympathetic, a few lines on its anatomy will not be inappropriate; both because it is necessary to have an accurate idea of what is to be studied, and because, as the microscope reveals more and more of the elementary structure of the body and its delicate arrangements, the function of organs is seen to be more intimately connected with their form and cellular composition.

The anatomy of the sympathetic is somewhat complicated, even when it consists in a mere description of the cords and ganglia; but becomes much more so when an attempt is made to follow the course of fibres from origin to distribution. The determination of paths of nervous influence, so far as dependent on the arrangement of anatomical elements, is really a portion of anatomy; and yet, in the present state of science, these paths are known more through physiological effects than by direct observation.

A strict limitation of the name ganglionic would perhaps exclude certain portions of the nervous system intimately connected in function with the nerves properly so called, namely, the vaso-motor fibres which are distributed with the cerebro-spinal nerves, and which leave the spinal cord with them, without previously passing through the gangliated cord; while, on the other hand, if all the ganglion-bearing nerves were included we should have to deal not only with all the posterior roots of spinal nerves, but with the sensitive root of the fifth pair, and with the pneumogastric and glossopharyngeal. The name sympathetic will then be preferred in this essay, not as indicating any theory, but as the least objectionable and most intelligible.

The system to be considered, which we call the sympathetic, consists then of the ganglion-bearing cord, situated on the sides of the vertebral column, from the superior cervical ganglion to the ganglion impar; of four ganglia in the head, ophthalmic, spheno-palatine (Meckel's), otic, and submaxillary; and of various large and small ganglia in the thorax and abdomen, with their connecting cords, beside the vaso-motor fibres accompanying cerebro-spinal nerves from their origin in the spinal cord, not having passed through the ganglia. These various parts constitute a physiological rather than an anatomical system, the former including the latter, with an addition of no very great magnitude though much importance. Our inquiries also include the tracing of the course of all the fibres connected with the gangliated cord, as well as of the direct vaso-motor fibres just mentioned, as far as possible in the cerebro-spinal centres, and the inquiry into the action of those centres, so far as manifested by means of the sympathetic system.

In order to avoid the introduction of too many details into the other portions of these pages, a short sketch of the histology of this system, as well as some general statements of the course of fibres, will be given here, the proofs of the correctness of many of the statements, being mainly physiological, appearing under the appropriate divisions.

The sympathetic system is, like the cerebro-spinal, composed of nerve-cells, nerve-fibres, and special peripheral terminations. The connective tissue furnishes for this, as for the cerebro-spinal system, an intimately connected support which plays an important part in the pathology.

The cells are contained in the ganglia, of which they are the distinctive feature. They have been said to be either apolar, unipolar, bipolar, or multipolar. It is not difficult to find in the ganglion of a cat, for instance, cells which will answer to any of these descriptions, but it is difficult to say positively, of a cell apparently apolar, anything more than that we can see no process from which a fibre probably arises. But a great deal depends on the preparation. If the cells are entirely separated by dilaceration, it is a matter of accident whether the processes break off, so as to show a distinct stump or not. The same thing may be seen in cells from other regions, for

instance the spinal cord, where we consider all the cells at least bipolar. If the cells are not separated, it is impossible to say how many processes are covered up; and if the preparation is made by section, we cannot be sure that one or two processes are not contained in the portion cut off.

Kölliker* says that although the existence of apolar cells has been formerly too easily admitted, yet there are cells without processes, especially in the sympathetic. He thinks, however, that they may be only stages of development of cells with processes. Polaillon† is more and more inclined to reject the idea of apolar cells, but cannot positively deny their existence. Duchenne‡ (de Boulogne) says very few cells are apolar. Seen in longitudinal sections they are multipolar, the most bipolar.

In regard to the unipolar cells, there is doubt, although Kölliker§ affirms their existence. The apparently single process seen in connection with the nerve-cells in the sympathetic ganglia of a frog, is shown by Beale‖ to consist of two fibres, one rolled spirally around the other, both of these being considered by him as of a nervous character.

Kölliker (loc. cit.) describes and figures the spiral fibres discovered by Beale, though without being quite sure of their nervous character in all cases. He has not seen the complicated arrangement described by Arnold.¶

The existence of multipolar cells is abundantly established. The nerve-fibres are of two different kinds, the dark-bordered and those of Remak, the latter being destitute of the medullary substance, and sometimes resembling connective tissue. Their precise structure is not altogether clear. The dark-bordered are of various sizes. It is the various admixture of these different kinds of fibres and of connective tissue which causes the lighter or darker shades of color in the branches of the sympathetic. "Some portions are white, like the main cord in

* Handbuch der Gewebelehre des Menschen, 1867, p. 255.
† Journal de l'Anatomie et de la Physiologie, 1866, p. 252.
‡ Comptes Rendus de l'Académie des Sciences, 1865, p. 131.
§ Ib., p. 326.
‖ How to Work with the Microscope, p. 238, and fig. p. 254.
¶ Kölliker, op cit., p. 253 and 254

most places, and the splanchnic nerves; others grayish white, like the intestinal nerves, and the nerves of the unimpregnated uterus; and others are gray and less tough, like the carotid and cardiac nerves, the branches connecting the great ganglia and plexuses of the abdomen, the branches to glands, and the pelvic plexus." *

Of the terminal arrangements of the sympathetic our knowledge is not so accurate or extensive as of the distribution of cerebro-spinal fibres to striated muscles, or the special peripheral arrangements of sensitive nerves. The nerves near their termination "consist of non-medullary, nucleated, embryonal fibres which, after free plexus-formation of the smallest trunks, and in part of the nerve-fibres themselves, finally end free."† Terminations of the motor fibres in smooth muscles, not very unlike some of those of the voluntary muscles, have been described by Klebs.‡

A proximate termination, a plexus, frequently containing ganglionic cells, and constituting in effect a ganglion adapted to the shape of the containing organ, is observed in several organs, and is probably of great physiological importance. Such have been described by Auerbach between the two muscular layers of intestine (plexus myentericus), and Meissner beneath the mucous membrane of intestines, these two communicating.§ His ‖ describes vascular nerves as ending in the tunica adventita and muscular coat in a plexus with nuclei, and a similar plexus in mucous membrane of urinary bladder. Arnold¶ describes a plexus in the iris.

Beale, Kölliker,*[1] and Lehmann also found pale fibres, probably motor, in the adventitia and muscularis of larger vessels of frog, and Beale and Lehmann found besides ganglion cells.

How far these data are applicable to higher animals is as yet uncertain. Some physiological facts seem to make it probable that the local ganglia are more highly developed in the frog.

* Kölliker, op. cit. p. 327. † Kölliker, op. cit., p. 329.
‡ Virchow's Archiv, Bd. XXXII., Heft 2.
§ Auerbach, Virchow's Archiv, Bd. XX., 2 and 3 Heft.
‖ Virchow's Archiv, Bd. XXVIII., 3 and 4 Heft.
¶ Virchow's Archiv, XXVII., 3 and 4 Heft.
*[1] Op. cit., p. 585.

The origin of sympathetic fibres in the cerebro-spinal axis is proved by many experiments and pathological data, which it would merely cause an unnecessary repetition to detail here, as they will be mentioned in connection with special nerves. It is also sometimes possible to trace them for a considerable distance beyond the spinal cord, and even beyond the ganglion with which they are at first connected. Beside these fibres there are many others originating in the sympathetic itself, and probably connected with the cells in the ganglia. It is chiefly, if not wholly, the finest dark-bordered tubes and all the gray non-medullary fibres which originate in this way.

What the primary centre for the sympathetic motor fibres is, or whether there is any distinctly limited centre, separate from the other motor ganglia, is not ascertained. It is undoubtedly above the medulla oblongata. The vaso-motor centre has been supposed to be in the crura cerebri,* but it certainly is not entirely uninfluenced by lesions higher up than this, as will be shown by pathological facts. The fibres pass downwards in the cerebro-spinal axis, probably in the anterior columns, leaving it through the anterior roots, and even through some cerebral nerves.† They then, with the exception of certain fibres which have been before mentioned, and which follow throughout the roots and trunks of the mixed motor and sensitive nerves, join the gangliated cord, and are thence distributed.

Whether the sympathetic fibres are connected or not in the cord with cells, has not been anatomically demonstrated, but on physiological grounds the affirmative is almost certain.

The assumption by Jacubowitsch of certain cells of the spinal cord as sympathetic, is apparently entirely unfounded. The very extreme improbability of an observer following, by any of the known methods, a nervous fibre from a sympathetic ganglion to a given cell, or to any cell in the spinal cord, is perfectly evident to any one who has worked enough over this organ to realize the difficulties in the way of following a fibre for a much shorter distance with certainty. Although, as I have said, the proof of the connection of the sympathetic fibres with some cells in the spinal cord is so strong as to

* Budge, Centralblatt f. d. Med. Wiss., 1864, No. 35.

† Nothnagel, Virchow's Archiv, Bd., XL., 1867, p. 203.

amount nearly to certainty, yet it is impossible to say which cells they are. It seems probable, from the experiments of isolating small portions of spinal cord, that these cells or secondary centres are situated near the place where the fibres arising from them make their exit from the cord, thus conforming to the probable arrangement of the other motor fibres.

Although only motor fibres have been spoken of as leaving the spinal cord through the anterior roots, the complete anatomy of the connection at each ganglion corresponding to the place of exit of the roots of the spinal nerves is not quite so simple as might be inferred from that statement. Each root of a spinal nerve has a branch communicating with the corresponding sympathetic ganglion, and in this communicating branch it is probable that fibres pass both to and from the spinal cord, that each root sends its appropriate fibres, the anterior motor and posterior sensitive, to the gangliated cord, and also receives back others which go to be distributed with the mixed trunk, or perhaps in part join the blood-vessels of the spinal pia mater.

The fibres which pass from the spinal cord to the sympathetic, though many of them small, are not the finest. These latter are found only in the trunk and branches of the sympathetic, showing, with a great degree of probability, that they take their origin in the ganglia. A fibre has occasionally been seen to arise from a ganglionic cell, but a large part of the fibres of the sympathetic trunk and the rami communicantes pass through the ganglia in bundles evidently forming no connection. As to the connection of these fibres of cerebro-spinal origin with ganglionic cells at any more distant point, it is entirely uncertain.

The distribution of the sympathetic is almost exclusively to the smooth muscular fibre, most especially to the viscera and the blood-vessels, and a few small muscles connected with the skin.

The exception to this rule which first presents itself is the heart; but the muscular fibres of this organ, though striated, differ considerably from those of voluntary muscles, and approximate in character the organic fibre-cells.*

* Kölliker, op. cit., p. 579.

The other exceptions are perhaps only apparent. The levator palati receives a branch from the spheno-palatine ganglion,* and the circumflexus palati and tensor tympani, from the otic.† Even supposing these branches to be the proper muscular nerves, and not vascular, the complicated anatomy of these ganglia would permit us to suppose the passage of the nerves for voluntary muscle through or near their substance to be an accidental arrangement, having no intimate connection with their function.

The sympathetic is to be regarded according to the foregoing description as a cerebro-spinal nerve, containing some fibres peculiar to itself, and distributed to the head and smooth muscular fibres. It differs from the other cerebro-spinal nerves in containing a large number of ganglia, at intervals, from near the beginning to the final distribution. It has a tendency to the formation of plexuses more or less complicated. The direct vaso-motor fibres are an exception to a part of this description, but are closely united to the remainder of the system by their distribution and function.

* Sharpey and Quain's Anatomy, Leidy, 1849, p. 270.
† Ibid., p. 277.

SECTION I.

THE sympathetic system includes, like the rest of the cerebro-spinal system, both sensitive and motor portions; the importance of the former, however, being, so far as we are conscious of it, much less than in those nerves which preside over our relations with the external world.

It is usually but a very slight perception which takes place of what is going on in regions of organic life. In cases, however, where the irritation becomes greater, more or less distinct sensations arrive at the sensorium, but never so distinct, however severe they may be, as those received through cerebro-spinal nerves.

The sensation of hunger is shown to persist after section of the pneumogastrics,* so that it must, so far as it is a local affection, be transmitted by the sympathetic.

The pains of colic, enteralgia, ovarian neuralgia, the passage of a calculus through the gall-duct or the ureter, are transmitted through this system.

One region supplied by the sympathetic, that is, the mesentery, contains a number of those curious sensitive apparatuses known as the Pacinian or Vaterian bodies. Their precise use is not known, and seems to be especially difficult to understand in this region. Their function has been supposed to be the perception of varying muscular pressure from the abdominal walls, as in the various movements of the trunk.†

Many parts of the sympathetic have been found sensitive to painful impressions; but experimental data are here of less value from the inability of animals to express any sensation but that of actual suffering, or to indicate any of the slighter differences of discomfort, to which we might suppose the irritation of this nerve likely to give rise.

The semilunar ganglia, when touched, or even strongly irritated to produce movements, give no sign of painful sensation,

* Bernard, Syst. Nerveux, II., p. 415.

† Rauber in Wien. Med. Wochenschrift (Review), 1867, No. 77.

and it is not until they are seized with the forceps and drawn upon to tear them out, that the animal feels pain and cries.* The cervical ganglia in the rabbit and dog are also sensitive to a slight degree, somewhat more so in the latter animal.† Meckel's ganglion cannot be pulled out without pain, though insensible to pinching.‡

Colin § found the ganglia all sensitive, the semilunar and thoracic more than the cervical. The sensations were immediately perceived if the excitation was strong; if weak, after some time. All the ganglionic nerves are sensitive, the smaller less so, and in the smallest cords there is almost no sensation. Those which communicate between the sympathetic and the cerebro-spinal centres are most sensitive, those uniting ganglia next, and those distributed to the viscera least.

Some of the arteries have also been shown by the same observer‖ to be sensitive to a ligature passed around them. These are the visceral arteries, those of stomach, spleen, liver, pancreas, kidneys, small and large intestines. The arteries of the head, neck, chest, abdominal walls, and limbs were not so.

The centripetal excitations which do not reach the sensorium, but which are converted into centrifugal stimuli, either in the spinal cord or in some of the ganglia, are much more numerous and also more important, since it is principally by their means that the motor impulses are put in action, and the harmony obtained which is observed in many of the movements, especially the peristaltic, which are under the control of this system. The supply of blood, as determined by the size of the blood-vessels, is also observed to be greatly influenced by reflex stimuli, and is undoubtedly, in the physiological condition, largely regulated by a stimulus, or the want of it, arising from the organ to be supplied, and passing through the nerves of sensation to a centre near or remote which controls the vaso-motor nerves.

As this form of centripetal excitation is so intimately connected with the motor functions, its further consideration will be deferred until the effects of excitation of motor nerves have been more fully stated.

* Bernard, Syst. Nerv., II. p. 522.
† Bernard, ibid., p. 492.
‡ Bernard, ibid., p. 95.
§ Comptes Rendus, 1861, p. 969.
‖ Comptes Rendus, 1862, p. 403.

SECTION II.

It is in the head that we are able to get the clearest and most decisive results in regard to the action of the sympathetic, and much of the knowledge of its action in other parts of the body results from generalization from this region. The reason is, partly, that we have access to the nerves controlling large and important organs without the necessity of causing, as is very likely to be done in the thorax and abdomen, intense inflammations which complicate or obscure the result, and we are thus enabled to watch effects for a much longer time. Besides this, the anatomy of the nerve is much simpler here than in the abdomen or thorax, where it is almost impossible to tell, anatomically, from how many different sources a given plexus may remotely take its fibres. In the limbs, which would otherwise furnish a most accessible field for experiment, sensitive motor and sympathetic fibres are united in a common trunk, and it is always difficult, and often impossible, to separate the effects of injuries of one class of nerves from those of another, when the trunk of the nerve is experimented on, and an operation on the component parts before they are united is by no means so easy or certain as a section of the cervical cord of the sympathetic.

If it were not allowable to reason from one part to another, the cases reported of changes of nutrition in limbs following injuries of nerves would be almost valueless physiologically, since it would be impossible to prove to which of the components of a mixed nerve the lesion should be attributed.

The classic and original experiment on the sympathetic consists in the division of the cervical cord, or the removal of the superior cervical ganglion; the first an easy operation, but unfortunately, in cats and dogs, a complicated one, since the trunk of the sympathetic is too closely united with that of the pneumogastric to be separately divided in the most easily accessible region; the second, from the deeper situation of the ganglion, somewhat more difficult, but without complication.

In the cat, the ganglia of the sympathetic and pneumogastric are united, but with a distinct line or furrow between them, and the first-mentioned may be removed without injury to the second.

The first and most obvious effects of either of these operations, as is well known, are—a contraction of the pupil on the operated side, partial closure of eyelids, a projection of third eyelid, and sinking of eyeball deeper into the socket, with increased heat and vascularity of corresponding side of head. These phenomena become apparent as soon as the animal is somewhat recovered from the ether.

Galvanization of the upper extremity of the nerve has the opposite effect to the section.

Beginning with the phenomena which were first observed and described, though without any intention of following a historical order, we shall have to consider:—

1st. Connection of sympathetic with movements of iris, eye, and, for convenience, a few smooth muscles connected with the skin.

2d. Influence of sympathetic on vascular system, and secondarily on blood, nutrition, and secretion.

3d. Influence of sympathetic on thoracic, abdominal, and pelvic viscera, except the vascular system.

The third topic, though not suggested by the experiment spoken of, completing the history of the sympathetic system.

SECTION III.

THE relations of the sympathetic to the movements of the iris have been variously explained, and although the fact of some connection is sufficiently obvious, yet the manner of it is by no means completely settled.

The principal observed facts in regard to the iris and its movements are as follows:—

1st. The existence of a sphincter pupillæ, a ring of smooth muscular fibres at the inner edge of pupil, is universally admitted. The dilatator pupillæ, described and figured by Kölliker, is said by Grünhagen* not to exist, and to be counterfeited by folds of the posterior surface of the iris. Donders† likewise expresses himself in some doubt as to its existence. Kölliker,‡ however, in his last edition reaffirms its existence in the rabbit's, and also in the human eye.

2d. Section of the sympathetic or removal of superior cervical ganglion produces contraction of the pupil, with projection of third lid and partial closure of upper and lower lids. This contraction is not immovable, but, on the contrary, the iris undergoes very considerable alterations, passing, in fact, nearly through the whole range of dilatation. This applies to cats. I have not seen any material diminution in size of rabbit's pupil after section of sympathetic. Grünhagen remarks that the movement of the rabbit's iris is very slightly affected by either operation.

In a cat from which I removed the superior cervical ganglion on the right side, all the above described phenomena were produced. The difference between the pupils was most marked in a light of medium intensity, while in a very bright sunshine

* Virchow's Archiv, Bd. XXX., p. 481.

† Refract. and Acc. of the Eye, New Syd. Soc. Trans., p. 578.

‡ Handbuch der Gewebelehre des Menschen, 1867, p. 667.

the difference was with difficulty perceptible, and was also less marked when the pupils were dilated to nearly their full size. Five days after the operation, when the kitten was excited by an attempt to remove the stitches in neck, the right pupil dilated the most, and ten days after, when etherized for excision of third lid, the iris on the side which had been operated on was much more widely dilated than the healthy one. The affected pupil not only accommodated itself to the amount of light in the room, but underwent the rapid oscillations which may be observed in the eyes of a healthy cat when a little excited.

3d. Galvanization of sympathetic causes dilatation of pupils. This has been observed in animals and confirmed for man upon the head of an executed criminal.*

4th. Section of third pair (oculo-motorius) is followed by dilatation of the pupil. This is, like the contraction from section of the sympathetic, not an unchangeable condition, for section of sympathetic counteracts it, and galvanization of the same nerve increases it, as does also the use of belladonna.†

5th. Direct irritation of third pair has been seen by Budge and Waller,‡ especially well in goats, then in dogs, rats, rabbits, and birds, and in executed criminals, to produce contraction of the pupil. This is denied by Longet,§ and by Bernard. Longet, however, proved, by another experiment, the action of this nerve on reflex stimulation. Bernard ‖ found a great difference in the effects, according to the place of the galvanization; for while he states galvanization of third pair in skull to produce no effect, the same process applied to ciliary nerves caused contraction. (Section of ciliary nerves in the dog produced extreme dilatation, while in the rabbit it produced contraction.)

6th. Section of fifth pair, or its ophthalmic branch, produces contraction of pupil, but not so marked as section of the sympathetic.¶ This contraction is not an immovable one. Further effects are vascularization of eye, and often, subsequently, destructive ulceration of the cornea.

7th. Irritation of fifth pair also produces contraction of pupil.

* Rud. Wagner, Journal de la Physiologie, vol. III., p. 175.

† Bernard, Syst. Nerv., II., p. 208, et seq. ‡ Grünhagen, loc. cit.

§ Grünhagen, loc. cit. ‖ Bernard, Syst. Nerv., II., p. 211.

¶ Bernard, op. cit., p. 85.

This is apparently a direct action, since it is unaffected by section of all (3d, 4th, 6th) the cranial motor nerves of eye.*

Donders,† thinking that this contraction might be due to fibres of the sympathetic which joined the fifth pair at the Gasserian ganglion, tried the following experiment: He exposed the sympathetic in the neck, and, after galvanizing it to make sure that it acted upon the iris, he divided the fifth pair in the cranium by the method of Magendie and Bernard, and after a time repeated the galvanization. In seven out of eleven cases, a dilatation of the pupil took place, but less marked than before division of the fifth pair. When the pupil was immovable, the vessels of the ear contracted on stimulation of sympathetic, while before division of the fifth pair it was seen that on stimulation of sympathetic, simultaneous dilatation of the pupil and contraction of the vessels of the ear occurred. This shows that all the fibres of the sympathetic do not follow the same course to the ciliary ganglion, but that some of them join the fifth pair, probably as far back as the Gasserian ganglion.

8th. The iris itself responds to the influence of light, perhaps without any nervous influence, certainly without any influence from beyond the plexus contained in its own texture.

The eyes of frogs react to light when entirely removed from head. Brown-Séquard ‡ says this action takes place in frogs and eels after so long a time that the nerves must have entirely lost their structure. When light is allowed to fall through a very small hole on the retina alone, without touching the iris, the pupil does not contract. When it falls on the iris alone, the pupil contracts. Contraction of the iris may also take place when the anterior is separated from the posterior portion of the eye.

A theory which shall completely reconcile all these facts, and explain beside, as it should do, the action of various drugs in causing mydriasis and myosis is not yet possible. Two explanations have been offered of the phenomena in question, and though both account satisfactorily, and probably correctly,

* Bernard, op. cit., p. 208 and 209.

† Refract. and Accommod. of Eye, New Syd. Soc. Trans., p. 578.

‡ Journal de la Physiologie, vol. II., p. 294.

for some of the movements, they cannot be reconciled with others.

The first supposes the iris to be regulated by two antagonistic nerves, of which one, the third, is distributed to the sphincter, the other, the sympathetic, to the *dilatator*. The second supposes the pupil to act principally by excitations transmitted from the retina, or perhaps on the direct stimulus of light to the surface of iris; either of these two organs being rendered more sensitive by section of sympathetic and consequent increased supply of blood.

The chief objection to the first is that there is no nerve whose section paralyzes the pupil or renders it immovable, either in dilatation or contraction. The doubt thrown upon the existence of a dilatator as a separate muscle is not of so much weight as might at first appear, since even those who deny its existence are obliged to account for the extreme dilatation observed on galvanization of sympathetic, by the action on the vascular muscles (Gefäss-muskulatur*), which thus are physiologically, if not anatomically, dilators of the pupil.

The second theory will not account for the extreme dilatation from galvanization of sympathetic, nor for contraction from irritation, direct or reflex, of third pair, unless we suppose what has, so far as I know, been neither proved nor suspected, that the third pair acts on the vessels of the eye in a manner opposite to that of the sympathetic, and similar to that of the chorda tympani on the vessels of the submaxillary gland.

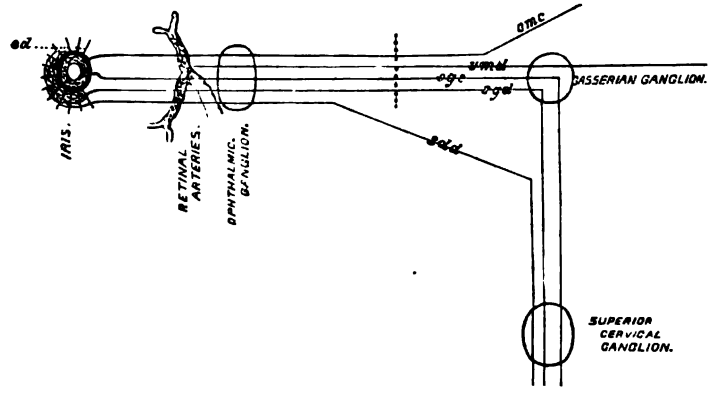
In all probability the movements of the iris are not to be explained on any very simple theory, nor on precisely the same theory, so far as the arrangement of nerves is concerned, for all animals which have been used for experiments.

The following explanation will account for nearly all the observed phenomena in rabbits. The series of experiments on cats is not so complete, and, if we leave the fifth pair out of the question, it is not so difficult to form a theory of the movements of iris in this animal, as there are fewer known conditions to satisfy.

* Grünhagen, loc. cit.

The usual and ordinary movements of the pupil are principally due to the sphincter under the control of the third pair acting against the elasticity of the iris. The sympathetic supplies to the eye three classes of fibres, dilating and contracting, which join the fifth pair at the Gasserian ganglion, and dilating fibres which join it nearer the eye, or pass directly to the ophthalmic ganglion. As before remarked, it makes no difference physiologically whether they are distributed to a special dilatator, or to the muscles of vessels which have same effect. The fifth pair contains vaso-motor fibres of direct origin from the pons varolii or medulla oblongata.*

It is possible to assign such values to the force of these different nerves, that this distribution will account for the effects of the principal experiments. The accompanying diagram will render the explanation more intelligible:—



Contractors.		*Dilators.*	
Oculo-motorius.	omc.	Sympathetic Gasserian dilator.	sgd.
Sympathetic Gasserian.	sgc.	Sympathetic direct dilator.	sdd.
		Elasticity.	ed.
		Vaso-motor.	vmd.

The vaso-motor fibres are reckoned as dilators because their action prevents, by moderating the supply of blood, the oversensitiveness of the eye to light, and thus opposes a reflex contraction of the pupil.

* Schiff, Untersuchungen zur Phys. des Nerven-system. Review in Jour. de la Physiologie, I., 209.

If now we assign the following figures to represent the relative force of the different nerves concerned,

omc = 10	sgd = 3	ed = 7.
sgc = 4	sdd = 2	vmd = 2.

we shall have omc+sgc=sgd+sdd+ed+vmd, i. e. 10+4=3+2+7+2 for a medium state of the pupil without experimental interference.

The experiments will then be represented as follows, section of a nerve being represented by removal of the corresponding term, and galvanic or other energetic irritation by doubling it, except in the case of the vaso-motor nerve. This exception is made because contraction of vessels does not cause anæsthesia so surely and rapidly as dilatation produces hyperæsthesia.

A preponderance of the first part of the comparison represents contraction of the second part dilatation.

Irritation of 5th pair, omc+2(sgc)> 2(sgd)+sdd+ed+vmd

10+8 > 6+2+7+2 18>17

Contraction.

Section of 5th pair, omc > sdd+ed 10>7+2

Contraction.

Irritation of sympathetic, omc+2(sgc)< 2(sgd)+2(sdd)+ed +vmd

10+8 < 6+4+7+2 18<19

Dilatation.

Section of sympathetic omc > ed+vmd

10 < 7+2

Contraction.

Irritation of sympathetic, after section of fifth pair between the Gasserian ganglion and eye, at the dotted line in the diagram, as in the experiments of Donders detailed above:

omc < 2 (sdd) +ed 10 < 4+7 10<11

Dilatation.

This represents the result in seven out of eleven cases. If we suppose (sdd) to have joined the fifth pair earlier, or to have been accidentally injured in making the section of fifth pair, it would represent the other four cases in there being no dilatation on galvanizing sympathetic.

In the cat, it is probable that the values assigned to the various classes of sympathetic fibres, including the vaso-motor,

should be increased, as all the phenomena on the part of the eye are more marked after section of the sympathetic than in the rabbit. All the phenomena seen to point in the same direction, that is, protection of the eye against excess of light, and are in a measure complementary to each other; thus the difference in the pupils is less when third eyelid is drawn over affected eye, and the difference in pupils becomes more marked after excision of a portion of this lid.

The ganglionic centres, from which arise the movements of the iris, are several. Perhaps the plexus in the iris itself may be the first, and cause the movements of the iris on direct stimulus by light. Although there is but little proof that the ophthalmic ganglion is a source of nervous power, or able to convert centripetal into centrifugal excitation, yet there are several circumstances which render it probable that such is the case. All the nerves which control the movements of pupil unite here with each other, and with nerve-cells. Experiment shows a modified action in the case of the third nerve, which does not when irritated excite the iris to action so readily as the ciliary nerves do. Section of all the ciliary nerves renders the pupil immovable, being in the dog dilated, in the rabbit contracted. Finally, in a similar ganglion which receives a twofold nervous supply, and distributes branches to the submaxillary gland, a degree of reflex action has been experimentally proved.

The encephalic centres for reflex movements are, however, the tubercula quadrigemina. Flourens, exciting directly one of the quadrigeminal or bigeminal tubercles, determined movements in the irides of both eyes. If one cerebral hemisphere is removed, the sight seems lost on the opposite side, but the reflex movements of contraction of iris continue on both sides, because the tubercula quadrigemina have not been injured. Finally, if the tubercles are wounded deeply, so as to abolish their functions, the pupil does not contract immediately under the influence of light, as in the normal state.* The further path of the excitations must be through the nucleus of the third pair.

* Vulpian, Leçons sur la Physiologie générale et comparée du Système Nerveux, p. 574.

The centre whence the excitations proceed, which traverse the sympathetic and induce dilatation of the pupil, is situated in the spinal cord, as shown by Budge and Waller, who still further localized this centre between the sixth cervical and fourth dorsal vertebræ. This region has received the name "cilio-spinal."

Brown-Séquard* assigns wider limits. He has ascertained that a section of one-half of spinal cord, as low down as fifth or sixth, and even sometimes ninth or tenth dorsal vertebra, affects the iris like a section of sympathetic, but in a less degree. He has, on the other hand, seen that some of the fibres animating the iris ascend the cervical part of spinal cord, and most probably go up to the medulla oblongata.

Bernard† considers that the phenomena on the part of eye, produced by section of cervical sympathetic, are independent of the vascular and calorific phenomena of head. He demonstrates separate anatomical paths of conduction for the two sets of phenomena by the following data:—

When the first two dorsal roots are cut, both contraction of the pupil, projection of third lid, and sinking of the eye are produced; but not vascular phenomena of head. The complementary condition, that is, the vascular effects without movement of the iris, is attained by dividing the sympathetic on the side of the vertebral column, between third and fourth ribs.

Brown-Séquard‡ does not agree with this view, as he expressly mentions the first and second dorsal nerves as giving exit to vascular nerves of head, besides, as before mentioned, having found that the iris is affected by roots far below these.

The partial closure of the eyelids and projection of third lid, which have been before mentioned, have received another explanation than that of which I have spoken. It has been thought that these phenomena were due to the sinking of the globe of the eye deeper into the socket. Pressure on the globe produces a forward movement of third lid, with great readiness; but although this may partially account for its position when

* Physiology and Pathology of Central Nervous System, p. 144.
† Comptes Rendus, 1862, p. 381.
‡ Phys. and Path. of Central Nervous System, p. 144.

the sympathetic has been cut, it does not account for its accommodating itself to the amount of light, so that the former explanation is of larger applicability.

The partial closure of lids, though perhaps also in a measure dependent on the increased sensitiveness of eye, recognizes another cause to which also the retraction of the eye is wholly due. The fibro-muscular sheath of Müller,* which is only very slightly developed in man, has the effect of thrusting the eye forward in the orbit, and its paralysis allows it to sink back under the influence of the recti muscles. This has been shown by direct galvanization and galvanization of sympathetic. The effect however does not take place through the spheno-palatine ganglion, as supposed by Müller, since it continues to be produced even after complete ablation of the ganglion.† A slight raising of lids upon galvanization of sympathetic, undoubtedly due to this muscle, was observed by Wagner ‡ in the head of an executed criminal.

There are other smooth muscles connected with the skin, especially around the ear of the cat, whose connection with the sympathetic, besides being probable from their structure, has been shown by galvanism (Müller, loc cit.). The subcutaneous smooth muscles found in the scrotum, penis, and perinæum, and also in the nipple,§ are probably also under the control of the sympathetic, if we may judge from the nature of their contraction as seen especially in the scrotum. To these may be added the arrectores pili, whose effect is seen in the production of "goose-flesh." It would be interesting for the transcendental anatomist to show the relation between the hair-bulbs thrust outward by the arrectores pili and that magnified and highly developed hair-bulb, the eyeball, protruded by its fibro-muscular sheath.

There are two ganglia which, for the sake of completeness and their neighborhood to the region we have been occupied with, may be considered here. But little is known physiologically of one, the spheno-palatine, and almost nothing of the other,

* Journal de la Physiologie, vol. III., p. 176, and vol. IV., p. 279.

† J. L. Prevost, Archives de Physiologie, No. 2, p. 231.

‡ Journal de la Physiologie, loc. cit. § Kölliker, op. cit., p. 98.

the otic. Prevost* says that the branches apparently arising from Meckel's ganglion are partly of ganglionic origin, and partly branches of the nerves which pass through it. Those of ganglionic origin are distributed to the nasal mucous membrane, internal maxillary artery, and also pass toward the fibro-muscular sheath of the eye, probably for its vessels. The Vidian nerve, which is partly composed of the great superficial petrosal, a branch of the facial, furnishes branches to the muscles of the palate. The ablation of the ganglion proves its insensibility (Bernard says it is not entirely insensible), and the integrity of the nutrition of the nasal mucous membrane, and of the sense of smell. Its galvanic excitation produces a notable elevation of temperature of corresponding nostril, and an abundant flow of transparent serum.

The operations of Carnochan† gave rise to no symptoms attributable to the loss of this ganglion.

The otic ganglion sends branches to the tensor tympani, but it cannot be shown that they are of sympathetic origin, and the rapid movements which this muscle must perform are not very similar to those usual in muscles supplied by the sympathetic. It is not improbable that the tensor tympani may really be supplied by the facial, as the velum palati seems to be.

* Archives de Physiologie, No. 2, p. 251. † Am. Jour. Med. Sciences, Jan., 1858.

SECTION IV.

THE next most noticeable phenomenon, after the section of the cervical sympathetic, is an increase of heat on the corresponding side of the head; and this is seen to be coincident with increased vascularity. The difference in a rabbit's ears, when the sympathetic has been cut on one side, is very striking, especially immediately after the operation. The affected ear is felt to be warmer by the hand, and there may be a difference of a number of degrees as marked by the thermometer; the amount varying somewhat, according to the temperature of the surrounding atmosphere and the degree of excitement of the animal. If the animal is loose, so that there is difficulty in catching it for examination, or it is excited by being handled, the normal ear often equals, or, as I have seen some days after the operation, surpasses in temperature the operated side. This difference of temperature is most easily recognized at the tips of the ears, that is, at the parts most exposed to the atmosphere. The difference in vascularity is easily seen in transparent parts, like the ears of rabbits, but is not so perceptible elsewhere, though the difference in temperature may be well marked. The vessels on the operated side enlarge to three or four times their usual dimensions, and, of course, surpass as much those of the other side, if, as remarked, the animal is quiet. The great variation in the amount of blood circulating in the ears of the rabbit, a variation which will be more particularly described below, renders some care and preparation necessary for appreciating the changes which section of the sympathetic has made.

It might naturally be supposed that the increased heat was directly caused by the larger current of warm blood passing through the ear; but it has been thought that the heating was only a secondary effect, at least partially so, of increased nutrition consequent on a freer supply of material.

The heat of the whole body undoubtedly depends on the chemico-vital changes taking place therein, and this heat is

continually being abstracted by an atmosphere varying from, in this climate very rarely, a few degrees above that of the body to more than a hundred (Fahrenheit) degrees below. Even when the temperature of the air is above that of the body, the increased evaporation has a constant tendency to cool the surface.

Although these chemico-vital changes are taking place throughout the organism, and although each remote portion contributes some heat to the common supply, it by no means follows that each part produces sufficient to suffer no loss from atmospheric cooling, and the warmth of a part depends on the heat of its own production, the influence of the surrounding medium, and the amount of heat which is brought to it from other parts of the body. The familiar facts that in cold weather mittens are warmer than gloves of the same material, and that the closed fist does not become cold so soon as outstretched fingers, illustrate this point sufficiently well. The fingers enclosed in a mitten produce, of course, no more heat than the same fingers separated, and the supply of blood is the same; but if the fingers are separated, the radiating surface is increased by that of the borders of the fingers, which in the mitten are in apposition. It is the same of the fist. The heat of the body is greater than that of the extremities and surface, both because the vital processes are going on with greater activity, especially in the abdomen, and because the surface, being much less in proportion to solid contents, the cooling is in the same proportion. When the surrounding temperature is much below that of the body, the extremities are constantly cooling, and usually faster than their own chemico-vital changes could make up for, so that the trunk is constantly supplying to them a greater or less amount of heat. That the carrier of this heat is the blood, and that it acts, beside supplying a certain nearly constant quantity of material for nearly constant nutritive changes—even in the case where its flow is increased by section of nerves—is, I think, shown by several facts.

That the amount of blood passing through a peripheral artery is able to modify its temperature is shown by the experiments of Dr. Lombard.* He shows the diminution of temperature over

* Archives de Physiologie, No. 4, p. 479.

the radial artery upon changing the rhythm of the respiration in such a way as partially to obstruct the circulation. An ingenious mechanical experiment was also devised by him to show the manner in which the obstruction of a tube through which warm water is pumped affects the temperature. His conditions were not arranged to correspond with our case, but they show that the temperature of a tube which is but little dilatable, through which a fluid warmer than the surrounding atmosphere is passing, depends on the amount of fluid which passes through a given space in a given time, and not on the absolute amount of fluid contained therein, which, of course, varies but slightly. If the tube is considerably dilatable, the accumulation also raises the temperature.

The plan of the circulation, in respect to heat, is not unlike that of the coil of iron pipe for heating an apartment with steam. The coil itself, being undilatable and incompressible, is somewhat in the condition of the capillaries, which are not contractile, or but slightly so, while the small arteries, with their rings of constricting fibres, are the physiological representatives of the supply-pipe with its throttle-valve. The coil, if tight, will always contain nearly the same amount of aqueous vapor, but at very different temperatures, corresponding to the opening of the valve, allowing a fresh supply to pass through, or its closure, and consequent cooling of the pipes, without the possibility of exchanging cool vapor for hot.

The temperature of an extremity, or of a rabbit's ear, seldom rises above that of the body, though Bernard * has frequently seen it at 40 degrees C. in the rabbit, while the normal temperature in the rectum of rabbit does not exceed 38 or 39 C. This may, however, be lower than that of interior of abdomen.

Bernard† further opposes to the simple theory of calorification his experiments of ligating the arteries and veins of an ear on the side where the sympathetic had been cut. He found increased heat on cutting the sympathetic where the veins had been tied, but not after complete ligature of arteries. Even in the ear, where the veins only had been tied, the temperature fell somewhat, although higher than the opposite side.

* Système Nerveux, II., p. 493. † Ibid., II., 508.

"D'après ces expériences, il n'est donc pas possible d'expliquer le rechauffement des parties par une simple paralysie des artères qui, à raison d'un élargissement passif, laisseraient *accumuler* une plus grande quantité de sang."

The explanation is not that heat is increased by accumulation of blood, but by its freer passage and more rapid renewal. The fact that the vascularity may diminish, and become not much greater than that of opposite ear, while the heat still remains greater, does not contradict this view, for if the smaller arteries are sufficiently dilated to permit a tolerably full stream to circulate freely through the capillaries, the heat will approximate its maximum, and a further dilatation of larger vessels, though making a great difference in the appearance of the ear, will have a less marked effect on the temperature.

The following experiments, though simpler than those of Bernard, show that the effect of greater or less vascularity in the ear is very similar in a healthy ear and in one for which sympathetic has been cut.

The figures given also show, beside the effect of interruption of the circulation, a gradual increase of temperature, which I have usually noticed after handling the ear.

A rabbit's ear that had been considerably handled, and subjected to one or two preliminary trials, so that it was very vascular, all the vessels being much distended, was fitted with a thermometer, the bulb being placed just within the tip of the ear, the edges of which were held closely around it with a couple of serrefines, not so tightly, however, as to interrupt the circulation.

Rises to 101¾.

The whole ear below thermometer was grasped with the fingers and tightly compressed. Thermometer *immediately* begins to fall, the ear becoming purple, and reaches 91°. It is still falling when pressure is removed, and temperature *immediately* begins to rise and reaches 102¼°.

On second compression falls to 99°, and still falling, when ear being released it rises to 101°+.

The temperature of this animal, between thigh and body, abour half an hour after the experiment, was 103°.

In a white rabbit the left sympathetic was cut, the right having been cut six weeks before, and there being but a very slight difference between the two ears before second operation.

TWO OR THREE HOURS AFTER OPERATION.

	L. ear 98+.	R. ear 80 about.
Strong compression of left ear, as in former experiment. Temperature rises about ½ degree, then falls to..... .	85	
Still falling when released. Then rises to.....	100½	
Before compression.....	100¼	
After.....	88	
Released.....	101½	R. ear 87.

These experiments seem to show that in Bernard's rabbit the ligature of two principal veins was not sufficient to stop the circulation.

Beside the observations on the temperature of the head and ear, and its connection with the vascularization of the parts, which is here easier to make out than in other parts of the body, there are others of the same character in the limbs and the great cavities, although they are somewhat more complicated in the performance of the necessary experiments.

The fact of the connection of increased heat with paralysis of the vaso-motor nerves is so fully recognized that it has been used as the principal symptom in determining the course of those nerves, both for the roots of the cervical sympathetic, and the more complicated anatomy of the nerves supplying the limbs and viscera.

The observations of Bernard, and especially of Schiff, show that a portion of the vaso-motor nerves of the extremities are contained in the gangliated cord of the sympathetic, while the latter experimenter has shown that another portion pass directly into the nerves of the limbs, directly from the spinal cord, and constitute that part of what, for the sake of physiological completeness, I have considered the sympathetic system, which does not pass through the ganglia.

Bernard * found that after section of all the roots of the sciatic nerve on one side, there was complete paralysis of sensation and motion, but no increased heat. On cutting the sciatic nerve at its exit from the pelvis, the temperature on the side operated upon was greater than on the other by 6° or 8° Cent. Something must therefore have been destroyed by the second section which was untouched by the first.

In another dog the lumbar sympathetic was cut. There was no paralysis of sensation or motion, but the leg experimented upon was 5° to 8° warmer than the other.

Schiff † observed the temperature of various parts of the limb, and arrived at conclusions differing somewhat from those of Bernard, but not totally irreconcilable therewith. A section of roots of sciatic produced heat of foot and lower part of leg; this was, however, increased by section of the trunk of sciatic. Section of three, or even five, last dorsal roots in the dog, without any lesion of spinal cord, always augments heat in the leg and inferior part of the thigh. Sometimes it produces warmth of foot and upper part of thigh. There are, however, cases where the fibres concerned are wanting in the thoracic region, and appear in the upper lumbar. This latter class of fibres belongs to the sympathetic.

The arrangement in the anterior extremity is analogous. Section of motor roots warms hand and lower forearm.

The vaso-motor nerves of upper arm, and upper part of forearm, come from the thoracic portion of spinal cord, lower than the nerves of voluntary motion. For the hand, a part of the nerves come from brachial plexus properly speaking, last three cervical and first two dorsal. They pass directly into axillary plexus. Another part comes from third, fourth, fifth, and sixth dorsal, and these, of which the origin is in common with the vascular nerves of upper arm, run like them through the superior thoracic portion of sympathetic.

The course of the nerves controlling the circulation of the abdominal viscera is not so well known as the number of experiments on these organs might lead us to suppose, but the characteristic effects of vaso-motor paralysis have been seen to fol-

* Comptes Rendus, 1862, p. 228. † Ibid., 1862, pp. 400, 425.

low section of certain nerves. Thus Samuel observed hyperæmia of intestines and liver after extirpation of cœliac plexus.

Hyperæmia of liver was observed by Haffter (quoted by Samuel) after section of great splanchnic.

There are but few facts, except those which are more strictly pathological, and usually somewhat complicated, by which we can apply observations on the sympathetic directly to man. Two experiments, however, by Dr. Augustus Waller,* showed the same effect after paralysis of the ulnar nerve by cold, as is seen in the limbs of animals after section of the nerve trunk supplying the parts.

In the first, the elbow was immersed in ice. After about an hour, when paralysis of ulnar nerve became complete as regards motion, the temperature of inner side of hand began to rise, and that of the last two fingers attained 36° Cent. The temperature of the fingers on the outer side of the hand became lower (28°). The inner side of the hand presented symptoms of active congestion, being red, very hot to touch, and pulse strong. The temperature of the mouth was 36°. The restoration of the normal temperature was more gradual than the return of movement and sensibility.

In the second experiment,† freezing mixture was applied over the ulnar nerve. The temperature between little and ring finger constantly rose for fifty-two minutes, when it had reached 33.8° and the experiment was discontinued. The temperature between ring and middle finger rose for about half the time, and then fell.

The opposite experiment to that with which we began consists in galvanizing the sympathetic in the neck, which produces, beside the effects mentioned upon the eye, the contraction of the blood-vessels of the corresponding side of the head, and a diminution of the temperature, if this had previously been raised by section of the nerve. Whether the condition produced in the nerve and the blood-vessels is precisely the opposite to that of paralysis, that is, whether the interrupted current, which has always been used for these experiments, is an accurately corresponding force to the normal stimulus, is

* Proceedings of Royal Society, vol. 11, p. 436. † Op. cit., vol. 12, p. 89.

doubtful, but it is enough for the present purpose to know that action of the sympathetic produces usually contraction of blood-vessels, and consequent diminished supply of blood, and diminished temperature; effects which are the opposites of those produced by its section. Contraction may be produced in the artery of a rabbit's ear by insertion of needles connected with poles of an induction apparatus nearly in its course, although it is possible that this may be a reflex action. Other instances of contraction of vessels from direct stimulus, where direct observation can be made, are seen in the iris* and ear,* from galvanization of cervical sympathetic. The effect of irritation of the sympathetic on the circulation of the submaxillary gland is very remarkable and decisive. It will be described in full hereafter.

The usual way in which the sympathetic is excited in the physiological condition is by reflex action, and many experiments also exhibit this form of stimulus.

Naumann† prepared frogs by separating one foot from the body, with the exception of ischiatic nerve. The medulla oblongata was separated, and the animal arranged so that the mesentery could be examined with the microscope. Upon irritation of the nearly amputated leg, the mesenteric vessels contracted, and circulation became more rapid. On application of stronger irritation the vessels dilated, and the circulation became slower. Similar phenomena were observed in the healthy foot and the vessels of the lungs.

The contraction of arteries of pia mater was observed by Nothnagel‡ on galvanization of crural nerve. This contraction, after removal of electrodes, gradually passes into dilatation.

Both contraction and dilatation of vessels of ear and of saphenous artery were observed by Lovén,§ on irritation of neighboring nerves.

The observations in which contraction or dilatation of large tracts of vessels is consequent upon reflex stimulus, as shown

* Donder's Refract. and Accom. of Eye, New Syd. Soc. Trans., p. 578.

† Eulenburg and Landois in Wien. Med. Wochenschrift, 1867, No. 64, from Prager Vjschr., 1863.

‡ Virchow's Archiv, 1867, 203, in St. Louis Med. and Surg. Journal.

§ Arbeiten aus der Phys. Anstalt zu Leipzig, 1866, p. 1.

by changes in the arterial tension, are very numerous, and will be given in more detail in another place.

An experiment of Brown-Séquard and Tholozan* showed that the same action took place in man; the temperature of one hand being much lowered by placing the other in very cold water; the intermediate parts remaining nearly the same. In repeating this experiment I obtained a reduction of 1¾° Fahr. between the fore and middle fingers of left hand, the right being immersed for ten minutes in snow and water, and a lump of the former held in the hand. The temperature of the apartment was between 70° and 80°.

The spinal centres and places of exit of the vaso-motor nerves of head and limbs have already been spoken of. Much more extensive vascular changes can be produced by operations on nervous centres than by acting on individual nerves. Brown-Séquard † draws a parallel between the effects of a section of a lateral half of spinal cord in dorsal region and section of the cervical sympathetic. They consist in dilatation of vessels, rise of temperature, and the other consequences of increased afflux of blood.

Ludwig and Thiry ‡ exposed by dissection a large number of arteries, which were found to contract on irritation of cervical portion of spinal cord. The contraction began sometimes in the smaller, and sometimes in the larger branches.

Certain of Bernard's experiments on the spinal cord involve the vaso-motor nerves, although it is not altogether easy to say how a section of spinal cord, or even a puncture (piqûre) above the brachial, but below the cervical plexus produces increased peristaltic movements of intestines, but diminished flow of blood through the abdominal organs. These might be symptoms of increased action of splanchnic nerves, the reflex power of cord being increased by section for these nerves as it is for the voluntary. Some other effects, such as the color of the blood in the portal vein, are the opposite of what we find on stimulus of sympathetic elsewhere.

A puncture of the floor of the fourth ventricle, in the middle of the space between the origin of the pneumogastrics and the

* Phys. and Path. of Nervous Centres. † Ibid., p. 145.
‡ Sitzungsbericht der Wiener Akademie, Bd. 49, p. 421.

origin of the auditory nerves, determines an augmentation of the quantity of urine, and the appearance therein of sugar. A little higher than this the urine is less abundant, less saccharine, but frequently albuminous.

A little below the origin of auditory nerves, the puncture causes increased flow of urine without sugar or albumen.* As these effects probably depend on modifications in the abdominal circulation, an inference might be drawn that the vaso-motor nerves for different organs are here, although near together, distinctly separated.

The effects in these cases, however, are too complicated, and the anatomy of the spinal cord and medulla oblongata is too unsettled, to allow a complete interpretation of all these facts as yet.

What the primary vaso-motor centre is, has not yet been determined. Pathology shows that lesions as high as the corpora striata and optic thalami affect the circulation on the side corresponding to the motor paralysis.

Liegeois † has approximated this centre for the frog. Regarding protrusion of the eye as due to increased vascularity, consequent excitability of certain muscles, and therefore to vaso-motor paralysis, he found that on cutting first one-half of cerebrum, and then the other, no phenomenon on the part of the eye was produced. On cutting one of bigeminal tubercles, exorbitis was immediately produced on opposite side. Upon one-half the bulb being engaged, the phenomenon took place on the same side. On cutting one-half the marrow at any height, no vascular change was produced. From this he concludes that the vaso-motor nerves in this animal originate in the tubercula bigemina, cross in the bulb, and are immediately distributed with the cranial nerves.

The experiments of Budge ‡ will hardly justify the assertion that the pedunculus cerebri contains the vaso-motor centre. They do not, in fact, even conclusively support the more limited claim which Budge himself makes, that by irritation of the pedunculus cerebri a contraction of all the arteries in the body

* Bernard, Syst. Nerv., I., 399.

† Journal de l'Anatomie et de la Physiologie, 1864, p. 62.

‡ Centralblatt für die medicinischen Wissenschaften, 1864, No. 35.

from head to foot can be produced. They consist in sinking two wires through the skull to the base of the brain, and conducting a galvanic current through them. The arteries of ear, leg, and abdominal wall may be seen to contract.

The possibility of the passage of the current beyond the parts in immediate contact with the wires, the well-known effects of reflex irritation on the vessels, and the uncertainty whether other parts of the brain may not have the same effect, render these experiments far from conclusive. The fact mentioned by Budge himself, that the effect extended to both sides of the body, lends weight to the first of these objections, and some experiments on the movements of the pelvic viscera, under the influence of stimulation to various parts of encephalon, justify the third.

The question cannot be regarded as settled.

SECTION V.

THE contraction and dilatation of a considerable tract of vessels, from irritation or paralysis of a portion of the sympathetic, are all the facts as yet stated in regard to its relations with the circulation. These, however, do not complete its vaso-motor physiology, even though leaving out of the question for the present its primary action on the heart. Other functions have been ascribed to the sympathetic in its vaso-motor capacity.

I quote the words of Eulenburg and Landois,* as their assertions are of importance if sustained, and as they state categorically what others take for granted or hint at:—

"The function of the vascular nerves consists in exciting the muscular apparatus of all the vessels, arteries, and veins, especially the first. The active movement of the vessels proceeding from the smooth muscular fibres is a double one, the pulsatile and the periodic, regulatory. The active pulsatile movement is to be recognized only in the arteries. As soon as a certain quantity of blood is thrown into the arterial system by the systole of the ventricle, and the elastic tubes have undergone an expansion, extending toward the periphery, with the swiftness of the wave of blood, the muscular apparatus begins its function, since the whole arterial passage actively contracts. The contraction begins immediately in the great vessels, and proceeds toward the periphery in the manner of a peristaltic movement, but with greater rapidity. The so-called pulse-beat or the passive systolic dilatation of the artery corresponds to the pulse which is felt, the succeeding falling together of the vascular tube is accompanied by the active contraction of the smooth muscular fibres, which are kept in rhythmic movement by the vaso-motor nerve-fibres.

"Besides this phenomenon of contraction depending on the muscles of the arteries, a second movement is observed in the blood-vessels furnished with muscles. From time to time one sees that the vessels of larger or smaller territories, gradually

* Wiener Med. Wochenschrift, 1867, No. 64.

and entirely, independently of the heart, beat, dilate, and then contract. This movement, discovered by Schiff, has for its object to furnish the organ concerned in which the vessels are distributed, now with a greater, and now with a less quantity of blood, as nutrition or external circumstances require."

"This is best recognized in the transparent ears of white rabbits, or the wings of bats, and can properly receive the name of periodic or regulatory movement."

Dr. Hartshorne,* principally on theoretical grounds, seems to recognize a movement like the first described. He thinks that the action of the muscles comes into play just after the elastic contractility, and assists in propelling the blood through the arteries.

Beginning with the last-mentioned of these two movements, it is in one of the regions mentioned, the rabbit's ear, a matter of such easy observation as hardly to deserve the name of a discovery.

Schiff,† as above stated, saw the periodic dilatations follow each other more or less rapidly, according to circumstances. Callenfells † observed the same phenomena of dilatation and contraction, but more slowly than Schiff, who, I think, mentions six or eight times a minute.

When a rabbit is watched, with little or no previous handling or excitement, there is but little change to be seen in the circulation. The ear is usually pale, with the larger blood-vessels marked out as pink lines, sometimes hardly perceptible. But as soon as the animal is excited or alarmed, the appearance changes. The main vessels, central artery especially, with its anastomoses, dilate to three or four times their former diameter, and a large number of vessels, before invisible, come plainly into sight. The whole ear is congested, covered with a network of red lines of varying thickness, and presents at a little distance, with the light transmitted, quite a bright, crimson appearance.

The artery can be felt to pulsate much more strongly than when vessels are contracted.

This condition continues for a few seconds, the interval varying at different examinations, and is succeeded by more or less complete contraction of vessels, seldom, however, reaching

* Amer. Jour. Med. Sciences, July, 1868, p. 287.

† Milne Edwards, Physiologie et Anatomie Comparée, vol. IV., p. 218.

the extreme point. Dilatation then again takes place, and so on. The condition of the ears generally nearly corresponds, but the dilatation is more lasting in the ear which is the most handled.

This phenomenon takes place, though in a less degree, after section of the sympathetic in the neck, the tendency then being toward the maximum of dilatation, the contraction being of less extent. The diagram will illustrate the difference between the oscillations of vascularity in a normal ear and those of an

ear corresponding to section of the sympathetic, the former being indicated by the dotted line, the latter by the black.

It is not probable that these changes are of much physiological importance; perhaps about as much as the similar phenomenon of blushing observed in our own species.

I have not had an opportunity of observing a bat's wing. The oscillations I have described are, however, not identical with the venous pulsation observed by Wharton Jones * in the bat's wing. He expressly excludes the arteries from any share in the phenomenon he describes, while in the rabbit's ear the increased volume is first noticed in the central artery. It seems to be a somewhat exceptional condition, as such rhythmic movements, unconnected with the heart, have been rarely observed. The vessels in the ears of the long-eared bat were not found by Wharton Jones to be endowed with rhythmical contractility in any marked degree, nor the veins of a mouse's mesentery.

The intermitting secretion of some glands is probably connected with a similar alternation of dilatation and contraction; the rhythm being much slower, the difference being that the circulation of the gland is usually under the influence of stimuli from some sensitive nerve, like the gustatory for the submaxillary gland, the pneumogastric for the stomach, and sensitive branches of the solar plexus for the abdominal viscera, while the ear depends on psychical or emotional excitations for controlling

* Philosophical Transactions, 1852, part 1, p. 107.

the condition of its vessels. That the secreting organs are not wholly withdrawn from the latter class of excitations, is shown by many pathological facts of perverted secretions from emotional causes.

The first-mentioned of the two forms of movement in the blood-vessels dependent on the action of muscular fibres is, from its supposed constant action in maintaining the physiological movement of the blood, a much more important phenomenon, if it exists. There are, however, many facts which show that it does not exist, and hardly one which cannot be as easily, and more accurately, explained on another hypothesis.

Many observers have long and carefully watched the current of the blood in the smaller blood-vessels of many living animals, and by none has an active, rhythmical, peristaltic movement of the arteries, synchronous with the heart's beat, been described. Pulsatile movements of the blood, corresponding to the impulse of the heart, are almost always to be seen, although in smaller vessels the current is so swift and so nearly uniform that a change in its velocity is not always to be made out. The fact of this kind of pulsation being described while the other is not, makes the negative evidence all the stronger.

Wharton Jones,* after carefully describing the pulsation of the veins, and expressly denying, as stated above, any pulsation of the arteries isochronous with that of the veins, makes no mention of any dilatation or contraction of the small arteries corresponding to the heart's beat.

Lister,† in a series of experiments on the circulation in the web of a frog's foot, and the dependence of the condition of the vessels on the nervous system, makes no mention of any pulsatile movement of arteries. He says, also, he has no doubt that in the frog the contractions of the heart are the only cause of the circulation.‡

Czermak § describes all the forms of pulsation in the blood of the mesenteric vessels of frog, from a swift stream in which an acceleration is hardly perceptible, to that in which the blood moves backward and forward without making any constant

* Loc. cit. † Proceedings Royal Soc., 1858, p. 607.
‡ Phil. Trans., 1858, p. 645.
§ Sitzungsbericht der Akademie zu Wien, Jan., Mar. 1865, p. 152.

progress, but has nothing to say of any movement of vascular walls to favor progress of blood.

Dalton* says "a pulsating character can be seen in the flow of the blood through even the smallest arteries under the microscope; but this pulsating character diminishes very considerably from the heart outwards."

In the mesentery of a small mouse, which was brought under the microscope, the current was at first so rapid in the small arteries near the intestine that I could perceive no pulsation, but blood pouring along in a continuous stream. After a few minutes, as the stream became slackened from approaching death, pulsations became visible in several arteries, varying in degree from a quickening of the stream to a backward and forward oscillation with no current.

In one artery very near the intestine, and for some distance nearly straight, where the current was quite rapid, I saw not only a pulsating acceleration of the blood, but an actual dilatation and contraction with each pulse, not very great in amount, but perfectly distinct. The movement was that of distention of the whole length of the artery in view at once, or so nearly so that the eye could detect no progressive motion.

These observations apply to such small arteries as can be brought under the microscope. The pulsation of arteries of larger size, from the aorta down to the lateral digital arteries, is well known. But it is in the larger arteries that the elastic tissue constitutes the principal portion of the walls, and in the smaller that the muscular structure predominates. If, then, the pulsation of the arteries is a combination of the heart-beat dilating and an active muscular action contracting them, and assisting in the propulsion of the blood by a peristaltic motion, as the intestines propel their contents, the phenomenon should be more marked as the muscular element gains the preponderance, and the arteries which are observed under the microscope should be seen to actively propel their contents. But, as we have seen, any pulsation of microscopic arteries is rarely seen, although their contents show rhythmical movements.

"The interrupted character of the arterial current, which is strongly pronounced in the immediate vicinity of the heart,

* Treatise on Human Physiology, p. 268.

becomes gradually lost and equalized during its passage through the vessels, until in the smallest arteries it is nearly imperceptible." *

The function of the vascular nerves, in maintaining by their activity a proper relation between the force of the heart and the resistance and capacity of the vascular system, is, as I shall hope to show, a very important one, but it is not an active propulsion of the blood in any way. There is no necessity for assuming anything of the kind, since the impulse of the heart is alone abundantly sufficient to carry on the circulation, and also fully accounts for all the pulsatile phenomena which are modified by the elasticity of the arteries.

Paralysis of the arterial coats—that is, paralysis of the vasomotor nerves—increases rather than diminishes the pulsatile character of the flow of blood. It is well known that the arteries on the side where the sympathetic has been cut pulsate more strongly than on the other.

Bernard † found, contrary to the statement of an advocate of the actively propulsive contraction of the arteries, that not only was the constant pressure increased after division of the sympathetic, but that the pulsations were stronger in a considerable proportion (80 to 60).

The experiment of Chauveau, ‡ alluded to by the same author, consisted in compressing the artery *beyond* the place of application of the instruments, so that the continued pulsation showed nothing of "an autonomous character, not simply dependent on the impulse from the heart," since there was nothing whatever to prevent the impulse being transmitted as far as the obstruction, just as usual, by the wave of blood.

The phenomena of the erectile tissues are brought forward in support of the active pulsation theory. Although, as shown by Legros, § the arterial walls are of great thickness, and contain only the smooth nerve-fibres of Remak, which may be assumed as parts of the sympathetic system, it is hardly pos-

* Dalton, op. cit., p. 268.

† Syst. Nerv., I., p. 485 and 229. Liquides de l'Organisme, I., p. 234, et seq. (several experiments); II., 12me Leçon.

‡ Onimus & Viry, Journal de l'Anatomie et de la Physiologie, 1866, p. 71.

§ Journal de l'Anatomie et de la Physiologie, Jan., Feb., 1868, p. 1.

sible that a simple increased action of these muscles should be productive of the whole phenomenon of erection. The anatomy is, beside this peculiarity, quite complicated, from the size of the dilated capillaries (areolæ), and also the singular dilatations in the course of the arteries (a very exceptional feature), which might be adduced to prevent the application of an active movement of arterial walls in this tissue, even if proved, to the remainder of the circulation. Rouget * considers the smooth muscles of the trabeculæ as essential a part of erectile tissue as the dilated blood-vessels, and ascribes to them the initiative in producing erection. The most conclusive evidence against the active propulsion of the blood, even in this exceptional tissue, is that furnished by the experiments of Lovén.† He found that galvanization of the "nervi erigentes" branches of the sacral plexus did indeed cause erection by an increased flow of blood; but instead of the pressure of the blood within the organ being greater than within the general circulation, as it should have been had the arteries exercised a directly propulsive force in addition to that of the heart, it was, in most cases, less than one-half of the pressure in the carotid artery; and in one case only, the canula of the manometer being applied to one vena dorsalis penis while the other was closed, did it reach sixty per cent.

The action of these nerves (nervi erigentes) is, like that of some other cerebro-spinal nerves, a simply relaxing one, and will be spoken of more at length in another place.

The calibre of a blood-vessel depends, within certain limits, upon two opposing forces, the pressure of the contained blood tending to enlarge it, and the greater or less contraction of the muscular fibres tending to make it smaller. The limits are much wider in proportion for the smaller vessels than for the larger, the latter being capable of very limited dilatation, and preserving a certain fixed size even after death, or the removal of all pressure from within, some of the veins, as those of the cranial bones and the cerebral sinuses, being nearly unchangeable in diameter. The limits are often narrowed pathologically by rigidity of the arterial coats.

* Journal de la Physiologie, I.

† Arbeiten aus der Physiologischen Anstalt zu Leipzig, 1866, p. 1.

There is no active dilatation. The variations in size of the smaller vessels are due to a varying muscular activity depending on the vascular nerves, acting against an also varying pressure of the blood. Mr. Lister * saw, as might be supposed, that the arteries do not dilate so fully when heart is very feeble, as when it is in powerful action. The heart of a frog having ceased to cause a forward movement of blood, he induced complete constriction of the arteries by irritating with a needle the posterior part of spinal cord, which (the cord) he then destroyed. The artery after this operation dilated to only one-half the size to which it had attained during former experiments when the heart was more powerful.

Position is often of considerable influence on the condition of the circulation in different parts of the body, as shown both experimentally and pathologically.

The tension of the blood in any given artery depends on that of the general system, and this again on the heart's force and the resistance of the vessels, so that the muscular activity of the walls of any one vessel not only affects its own calibre, but, indirectly, that of all the other vessels in the body. This is, of course, only perceptible when some considerable portion of the vascular system is affected.

The nervous system, then, influences not only the local, but the general circulation, the heart and the arteries each being directly controlled by it, and also indirectly acting on the other.

The part of the nervous system which exercises this function is the sympathetic, using the word with the meaning I have previously given it, and also certain cerebro-spinal nerves, which act in a manner different from, and sometimes apparently opposed to, that of the sympathetic. These latter are positively known only in a few instances. Whether there are nerves of similar function throughout the body, corresponding in distribution with the vaso-motor portion of the sympathetic, future inquiries must determine.

The effect of extreme dilatation (want of "tonus") of a considerable portion of the vascular system on the action of the heart, and on the remainder of the circulatory apparatus, has

* Proceedings Royal Soc., 1858, p. 607.

been shown by Goltz.* His "Klopfversuch" † consisted in exposing the heart of a frog, and then repeatedly rapping the abdominal walls. The first effect was a gradual slowing and final stopping of the heart's beat. As the heart recovered from this reflex stoppage, its action was far from normal, showing by its flaccid condition, pale color, and small quantity of blood sent into the great vessels, that it was very imperfectly supplied with blood. The venæ cavæ were almost empty, and the circulation in the webs of the feet was completely stopped. These phenomena were made more evident by placing the animal upright. In the course of half an hour the pulsations of the heart resumed their normal character.

Without describing in detail all the modifications of this instructive experiment, it may be said that the secondary effects are in no way consequences of the primary, since it is possible, by beheading the animal, or cutting the vagi, to separate the two phenomena, and observe the insufficient blood-supply of the heart, without its previous stoppage. Neither is hemorrhage the cause, although the appearances are so similar, for the experiment, properly performed, produces no material rupture of vessels. Upon opening the abdomen, however, all the vessels are found to be greatly distended with blood, contrasting strongly with the anæmia of neighboring regions; and it is this local withdrawal of blood which produces temporarily on the heart, and on the vessels of the feet, the effect of its removal from the system. Further experiments show that as the vessels contract under the influence of the central nervous system, the blood is furnished in greater abundance to the heart, which then gradually resumes its normal pulsations.

The wider limits within which the capacity of the visceral veins of the frog varies than that of the arteries, and also an experiment in which the aorta was ligatured, show the important part which the veins take in this phenomenon, and

* Virchow's Archiv, Bd. XXIX., p. 394.

† This is very incorrectly translated "crushing blow experiment." Klopf does not mean crushing blow, and it is very evident that a succession of "crushing blows" on a frog's abdomen would leave very little circulation of any kind capable of observation.

also their immediate dependence on the central nervous system, their dilatation not being merely a consequence of increased supply of blood from the arteries.

Either the spinal cord or medulla oblongata were found sufficient to restore the contraction or "tonus" of the abdominal vessels; but after removal of both the distention was permanent.

It is not probable that anything corresponding to this extreme case can be observed in man or the mammalia; but the condition of the abdominal circulation has been shown to influence in a very marked manner the pressure of the blood in the carotid artery. Thus Asp * found after section of the great splanchnic nerve in dogs, that the pressure of the blood in the carotid was considerably diminished, while galvanization of same nerve increased it. Cyon and Ludwig † found irritation of the splanchnic nerve to increase the tension even beyond its normal value. These last-mentioned experimenters also found the tension in carotid diminished by irritation of one of the sensitive nerves of heart, and that this diminution of tension coincided with an increased supply of blood to abdomen, was shown by observations of a change of color in the kidneys.

We have heretofore spoken explicitly only of an excitation of the sympathetic, either direct or reflex, causing contraction of the vessels, and of a paralysis caused by its section and permitting dilatation. The foregoing experiments, and some others of great importance, demand a more particular reference to the direct or reflex dilatation of vessels, which takes place without a section of the corresponding vaso-motor nerves having been made.

A primary dilatation of vessels may take place either from direct or reflex stimulus. While we cannot understand precisely how a stimulus which we may call positive, and which applied in one way would cause contraction, becomes converted into a negative one, if such an expression be admissible, and produces dilatation, there is no more absurdity in supposing such to be the case, than there is in supposing an arm, for instance, which holds out a weight horizontally, at liberty either

* Arbeiten aus der Physiologischen Anstalt zu Leipzig, 1867, p. 131.

† Arbeiten aus der Physiologischen Anstalt zu Leipzig, 1866, p. 128.

to raise or lower it at the command of the will, the one requiring increased, the other diminished action, or none at all. Much depends on the degree of irritation, but it is extremely doubtful whether we have a right to refer primary dilatation to exhausted irritability. The theory does not answer for all cases, and observation shows that dilatation may take place without any perceptible previous contraction.

Four ways may be distinguished in which vascular dilatation takes place besides section of vaso-motor nerves.

The first is dilatation after previous contraction; the second is primary, from direct irritation; the third depends on irritation of a motor, and the fourth on irritation of a sensitive cerebro-spinal nerve.

Examples of the first are the dilatation of vessels in bat's wing after slight mechanical stimulus with the point of a needle,* probably the alternate dilatations and contractions in the vessels of rabbit's ear described above, and, to borrow an example from pathology, the fever which follows a chill.

The second method is seen in the bat's wing after chemical stimulus.† Schiff‡ observed a local dilatation from tickling artery in rabbit's ear. The effect was the same after cutting sympathetic, or extirpating superior cervical ganglion. If sensitive nerves were cut, the same tickling did not produce the same effect. After section of all the trunks, a little harder rubbing acted in the same way. It is probable that a part at least of the vessels concerned in inflammation dilate without previous contraction.§

For the discovery of the third form of dilatation we are indebted to Bernard.‖ His experiments upon the submaxillary gland of the dog consisted in so dissecting it that the artery, vein, duct, and two sets of nerves were exposed. The branches of the sympathetic accompanied the artery, and were joined at the submaxillary ganglion by branches from the gustatory

* Paget's Surgical Pathology, p. 198. † Ibid., p. 199.

‡ Comptes Rendus, 1862, p. 541.

§ Paget, op. cit. p. 200; and Cohnheim, St. Louis Med. & Surg. Journal, March, 1868, p. 129, from Virchow's Archiv.

‖ Système Nerveux, tom. II., leç. 7; and Liquides de l'Organisme, tom. leç. 14, and tom. II., leç. 12, 13.

branch of the fifth. These branches were found to belong really to the chorda tympani, a branch of the facial, and incidentally, as it were, to follow the course of the gustatory. The action of the two nerves was found, in numerous experiments, to be in some respects precisely opposite. If both were cut and successively galvanized, the sympathetic caused almost complete cessation both of the flow of the blood and of the secretion, the latter being, in the cases where a few drops were collected, extremely viscid, and the blood being very dark. Excitation of the chorda tympani, on the other hand, produced a rapid flow of blood, which issued from the veins red, and sometimes pulsating, while a watery secretion flowed copiously from the duct.

The nerves which have a corresponding effect on other glands have not yet been made out.

There are two theories of the action of the chorda tympani on the submaxillary gland, somewhat differing from that which I have here adopted. One assumes an active dilatation. This is unnecessary, since the pressure of the blood is sufficient to bring about the dilatation, if the resistance of the muscular fibres is removed. It is difficult to see, also, how a single layer of muscles, such as exist in the smaller arteries, can act in two opposite directions.

The second is, that the chorda tympani acts directly on the secreting cells of the organ, and, by the impulse given to their functional activity, causes a demand for more blood.

One of the best answers to this supposition is found in the phenomena of erection as observed by Lovén,* and described above, since we see in this case the same acceleration of the blood-stream, without there being any secreting organs whose activity could call for an increased supply of nutriment. Lovén found that galvanization of the nervi erigentes caused erection of the penis, and showed that this was due to increased flow of blood. The pressure of the blood, however, never reached that of the carotid artery. The nervus pudendus contains not only the vaso-motor fibres for the erectile tissue, but the fibres for the smooth muscles of the trabeculæ, and is doubly an opponent of the nervus erigens.

* Op. cit.

One of the most important facts in regard to the double nerve supply of certain organs, of which the number known will probably be some time much increased, is that it is connected with ganglia near the terminal distribution. The sympathetic fibres and the chorda tympani unite in the submaxillary ganglion, which then gives off branches to the gland. Lovén has demonstrated the existence of several small ganglia connected with the nervi erigentes, and probably with the sympathetic also, and the heart, which, as we shall see, has also a double, and, in a measure, mutually opposing nervous supply, is richly supplied with ganglia.

It is fully consistent with the present well-founded theories of the manner in which one nerve is able to influence another, and especially of the production of reflex motion, to suppose that it is in the nerve-cells of these ganglia that the influence of the cerebro-spinal nerves is opposed to that of the sympathetic, and not in the walls of the vessels themselves; and it is reasonable to infer that the action of the former consists simply in neutralizing the activity of the latter, which, unopposed, produces contraction.

If to this it should be very justly objected that while section of the sympathetic roots of the submaxillary ganglion increases the flow of blood through the glands, yet stimulation of the chorda tympani, subsequent to this, increases it still more, we must seek the explanation in the ganglionic structures which still remain connected with the gland. Of these the first is the submaxillary ganglion itself, which we must, according to the present theories of the production of nervous force, consider a subordinate source of sympathetic activity, although probably of short duration. It has also been experimentally proved to possess a degree of reflex power.

The plexuses formed by the vaso-motor nerves in the walls of the vessels contain nuclei, and may thus be considered to be connected with more or less perfect cells.*

That these or some other ganglionic structures, situated beyond the cerebro-spinal centres, and beyond the great ganglia of the trunk, possess the power of causing contraction for a long time after separation of a part from the primary centres, is

* His, Virchow's Archiv, Bd. XXVIII., p. 427.

shown by the experiments of Lister.* He amputated the leg of a frog after previous ligature and watched the vessels.

For several days there was observed a variation in the calibre of whole vessels, and of different parts of the same vessel. He once saw an artery, in the act of slow contraction at one part, driving the blood into a dilated portion at a little distance. During the ninth day after amputation, variations in calibre continued to show themselves, but the next day the vessel observed had an almost uniform width, and other evidences of loss of vitality in the tissues began to show themselves.

It is by both these sets of ganglia that a degree of tonicity is maintained in the vessels of the submaxillary gland after section of sympathetic roots of the ganglion, and these last remains of nervous force are counteracted or overpowered by the excitation of the chorda tympani.

The fourth way in which dilatation is produced is by irritation of a sensitive nerve. This is often, perhaps usually, a secondary dilatation, being preceded by contraction, and often reflex, as in the case of irritation of the gustatory nerve, where a sensation from a sapid body applied to the tongue passes to the brain by the sensitive nerve, is there reflected to facial and chorda tympani, and arrives at the submaxillary ganglion to oppose the action of the sympathetic. Although these two exceptions include the larger part of the cases of dilatation following excitation of a sensitive nerve, yet there are a few in which the dilatation seems primary, and the reflection of the stimulus appears, so far as we can see, to take place in the cerebro-spinal centres.

Lovén † found, in a number of animals poisoned with curare, that irritation of the central extremity of a divided auricular nerve caused dilatation of vessels of ear without previous contraction. The same was the case with the saphenous artery on galvanizing the nervus dorsalis pedis. The phenomenon, however, seems to have been a somewhat uncertain one, and the dilatation was soon followed by contraction.

Cyon and Ludwig ‡ galvanized the central extremity of a branch of the vagus which is distributed to the heart, which

* Proceedings Royal Society, 1858, p. 607.

† Arbeiten aus der Physiol. Anstalt zu Leipzig, 1866, p. 1.

‡ Ibid., p. 128.

they named nervus depressor. They found that immediately after the beginning of galvanization the arterial pressure began to fall. The same took place after section of the vagi. After section of both splanchnic nerves the same effect was seen, but in a much less degree. Galvanization of splanchnics produced the contrary effect. From these data it is inferred that irritation of the nervus depressor produces a relaxation of the abdominal vessels, and consequent diminution of the pressure in the general circulation. This relaxation is primary, and principally takes place through the splanchnic nerves.

It is probable that the relation between the sensitive, motor, and vaso-motor nerves is essentially a much more constant one than at first appears, that is, there is some one law which regulates all the vaso-motor reflex movements, whether of dilatation or contraction. What the law is, however, it as yet impossible to determine; but the differences observed in the actions of various nerves are undoubtedly dependent in a great measure on degree and kind of irritation. This can be shown experimentally, as regards degree, for certain allied phenomena to those we have been describing, and very important differences in the action of the sympathetic have been seen to depend on the kind of galvanic current used for excitation.

Thus Onimus * found that continued currents applied to the great sympathetic determine augmentation of temperature, which is greater and more lasting the longer and stronger the electrization has been employed. It is never so high as in cases of paralysis of sympathetic, and never surpasses the normal temperature. Neither in animals nor in man does the calorification take place until some time after galvanization, usually from one to three hours. Onimus believes this to be due to excitation of vaso-motor nerves which favor the autonomous contraction of arteries.

The contractility is augmented, but no spasm or permanent contraction is produced. "The smooth muscular fibres continue to have the vermicular contraction, only this kind of contraction, which belongs to them, is exaggerated, and consequently the progress of blood-current is favored."

"The continued current never produces permanent contraction

* Comptes Rendus, t. LXV., p. 250.

in either kind of muscles, but even causes the tetanic contraction obtained by intermitting currents to cease, as well as the tetanic contractions from poisoning by strychnine."

If our views as to the function of the vaso-motor nerves are correct, it is rather in a general relaxation of the muscular coat of the arteries that we are to seek the cause of the increased heat than in any peristaltic movement. It is evident, however, that the effect of the two kinds of current is very different.

The opposite effects obtained in local irritation, according to the kind of stimulus employed, will also be remembered, as in Wharton Jones' experiments.

The duration of the effects obtained upon section of the sympathetic, or extirpation of a ganglion, is different, according to the operation done, and probably according to the individual or species of animal operated on. The effect of simple section of sympathetic is shown at first by extreme dilatation of vessels, but within a few hours is limited to a much less amount of vascularity, and after some days, or two or three weeks, disappears almost entirely. The removal of the upper cervical ganglion produces more durable effects. The final restoration of the normal vascular condition of a rabbit's ear, after section of sympathetic, might be supposed due to reunion of the divided nerve. It is more reasonable, however, to attribute it to the same cause which produces the primary reduction of congestion, and this explanation would hardly suit a case in which three-quarters of an inch of the nerve was taken away.

The restoration is probably due to nerve-fibres which reach the affected vessels through other channels than that which has been destroyed. Such are, in the rabbit, the roots of the superior cervical ganglion coming from the upper portion of spinal cord. The cervical nerves also control vessels of ear.* The part of the terminal plexus influenced by the fibres still intact, not being distinctly limited from that connected with the fibres divided, gradually increases, until the corresponding nerve possesses the full powers of the paralyzed portion. The removal of a superior cervical ganglion, by cutting off nearly the whole nervous supply, makes the effect not only more marked than that of section of sympathetic, but much

* Schiff, Review in Journal de la Physiologie, Vol. I., 209.

more lasting, being, in some animals, of indefinite duration. The small amount of connection requisite to maintain the control of the nervous centres over the circulation, is shown by the experiments of Lister.* In twenty-four hours after section of one sciatic nerve in frog, both feet became alike, and varied alike, according to its struggles.

The results of several experiments in which he made nearly a complete division of sciatic or its roots, being a restoration of contractility, while removal of spinal cord was followed by complete and permanent dilatation, led to an inference which I have stated above, that remaining fibres perform the functions of those which are divided.

He cut off a whole leg, except bone, artery, and vein. The first effect was full dilatation of the vessels, which, after twenty-seven hours, resumed a moderate size, the circulation being still active. He then examined carefully with microscope the coats of artery and vein, and also the periosteum, with a very slight amount of muscular tissue adhering, but could detect no nerves. Yet from comparing this condition of the vessels with the permanent dilatation observed on removal of spinal cord, it was evident that slender filaments had served to keep up communication. In another case in which this experiment was performed, after removal of the spinal cord, the arteries remained dilated.

The difference between the complete paralysis caused by removal of spinal cord, and the irregular contractions in the completely amputated leg, mentioned some pages before, is not easily to be explained. Destruction of the nervous centre seems to cause paralysis even more permanent than that which results from complete separation from the centre.

It seems as if there were a collateral vaso-motor supply, after section of a nerve trunk, not unlike the collateral circulation after ligature of an artery.

* Proceedings Royal Society, 1858, p. 607.

SECTION VI.

THE circulation is under the control of the central nervous system in two ways.

The first, which has just been described, is through the vaso-motor nerves; the second, through the cardiac branches of the sympathetic and pneumogastric; the sympathetic in each case favoring the contraction of the muscular walls of the organ to which it is distributed, in the one case constant, in the other rhythmical. The different parts of the sympathetic are thus, to a certain extent, opposed to each other, although the action of each part is required to maintain the necessary conditions of the circulation, that is, a due proportion between the volume of the circulating fluid and the capacity of the containing vessels. The resistance to the passage of the blood due to the activity of the vaso-motor nerves increases the arterial tension. It has been thought also to decrease the number of the beats of heart; but various experiments have shown that the irritation of certain nerves which increase and decrease the tension of the blood, and at the same time increase or decrease of the number of pulsations, acts independently on the arteries and on the heart, and not always in the same way. Thus Asp * found in nearly all the cases in which, after irritation of the splanchnic nerves, the arterial tension rose, that the pulse fell. But in several experiments where the central extremity of the lumbar spinal cord was irritated, the pulsations rose as well as the tension.

The effect on the heart of mechanical stimulation to the lumbar spinal cord was opposite to that of electrical, although the tension was affected in the same way.

The irritation of muscular nerves, although uniformly increasing the tension, in some cases increased, in others diminished, the rapidity of the heart's beat.

* Arbeiten aus der Physiologischen Anstalt zu Leipzig, 1867, p. 131.

After removal of the ganglia stellata (last cervical and first dorsal), which furnish the principal cardiac branches in the rabbit, the effect on the heart was found to nearly or quite disappear, but the tension was still affected.

Ludwig and Thiry's* experiments lead to a similar conclusion. They found that irritation of cervical spinal cord caused contraction of many blood-vessels throughout the body, and an increased tension. The pulsations of heart were, however, variously affected, being sometimes increased and sometimes diminished in number. After destruction of ganglia stellata, which they accomplished with the galvano-caustic, the effect remained the same in character, sometimes increasing, sometimes diminishing the number of pulsations.

Cyon and Ludwig† have, however, shown that a reflex dilatation of abdominal blood-vessels produces an effect approximating that in Goltz' experiment, where vascular relaxation, so great as not only to offer but slight resistance to the passage of the blood, but also to prevent its due return to the heart, was shown in the peculiar powerless pulsations of that organ.

The heart is supplied, like some portions at least of the vascular system, with nerves from two sources, the sympathetic and pneumogastric, which have a different action; and the description of one would not be complete without that of the other.

The sympathetic is the motor nerve. Galvanization of the sympathetic, especially of superior thoracic ganglion, has, according to Valentin,‡ been found to accelerate the beats of the heart in rabbits, or to excite the motion anew if it has ceased.

Von Bezold § considered that three-fourths of the normal propulsive force of the heart reaches it from the spinal cord, between the seventh cervical and third or fourth dorsal vertebræ, passing probably through lower cervical and upper dorsal sympathetic ganglia.

E. and M. Cyon ‖ found the third branch of the inferior

* Sitzungsberichte der Wiener Academie, 1864, 2, p. 421.

† Arbeiten aus der Physiologischen Anstalt zu Leipzig, 1866, p. 129.

‡ Grundriss der Physiologie, p. 715. § Am. Jour. Med. Sci., July, 1863.

‖ Comptes Rendus, 1866.

cervical ganglion in the rabbit, and the second branch of the same ganglion in the dog, to increase the number of the heart's beats. They propose to call these the accelerator nerves. Moleschott * says that a slight irritation accelerates the heart's beat, while a strong one diminishes its frequency, and may even cause temporary suspension.

The effect of strong, or even moderate, excitation of the vagus is well known. It diminishes the number of pulsations, and sometimes completely stops the heart in a condition of diastole. This condition of the heart is a passive one, and is not tetanic. The prick of a needle which touches the dilated ventricle is followed by a pulsation of whole heart,† just as happens upon exciting in the same way a heart which has ceased to beat, after being for some time removed from the body.

The results of slight irritation of vagus, on the contrary, are to accelerate the heart, as stated by Valentin,‡ Moleschott,§ and Schiff.‖

Onimus ¶ found the constant current not to produce arrest of heart. When very strong the heart's beats were much less energetic, but this was due to troubles in respiration. After pneumogastric was divided, the galvanization of peripheral extremity by an ascending current produced no effect, while a descending one rendered pulsations more frequent. Von Bezold*[1] has not observed acceleration of heart from galvanization of vagus, but a tetanic state proportional to intensity of current.

The portion of the vagus which produces these effects has been found to be that portion of the internal branch of spinal accessory which arises from medulla oblongata.†[1]

Beside these two systems which connect the heart with the cerebro-spinal centres, and which are themselves connected in the cardiac plexuses, this organ possesses another partially independent system of ganglia in its own substance, which perform a very important part in its physiology.

These ganglia are widely distributed through the muscular

* Journal de la Physiologie, V., p. 124.
† Valentin, op. cit., p. 715. ‡ Op. cit., p. § Loc. cit.
‖ Comptes Rendus, 1861, p. 285. ¶ Ibid., t. LXV., p. 250.
*[1] Journal de la Physiologie, V., p. 293.
†[1] Heidenhain, Studien des Phys. Instituts zu Breslau, 1865, p. 109.

substance of heart. Friedländer* demonstrated the existence of one or more cells in every fragment of heart which continued to beat, even if consisting of very few muscular fibres.

Their anatomical relations with the nerves are not yet settled, Kölliker† denying their connection with the vagus, and Friedländer doubting the correctness of this statement. The physiological connection of the several systems, though not clearly explicable, is most undoubtedly a real one.

It is very well known that neither the vagus nor the sympathetic is necessary to the performance of a single pulsation, nor even of a considerable series of pulsations. The hearts of many cold-blooded animals—reptiles, amphibia, and fish—beat for a long time after removal from body. The heart of a rabbit continued to beat after section of both vagi and extirpation of ganglion stellatum with its branches, and the thoracic portion of sympathetic as far as second rib.‡ Czermak and Piotrowski found the hearts of rabbits to beat for over half an hour after removal from body.§ The conclusion can hardly be avoided, that the system immediately presiding over the pulsations of the heart, and without which they could not take place, is that contained in the organ itself. The sympathetic, although a motor and accelerator nerve, is not the immediate excitant of the movements of the heart. It rather supplies nervous force to the ganglia, which is by them used in producing the pulsations.

The relations of the pneumogastric to the sympathetic and to the heart are explained on two principal theories, the restraint (Hemmungs) theory and exhaustion (Erschöpfungs) theory.

The first expresses the well-known facts, that section of the vagus increases the number of heart's beats, and that its irritation diminishes it. The second claims the vagus as a motor nerve of the heart, since its slight irritation quickens the pulsations. Its action in causing stoppage is explained by supposing its over-excitement and consequent exhaustion to be communicated to the other nerves and the ganglia. The im-

* Archives de Physiologie, in review of "Recherches faites dans le laboratoire physiologique de Wurzburg," 1868, p. 339.

† Op. cit. ‡ Cyon and Ludwig, op. cit., p. 135.

§ Sitzungsbericht der Wiener Akademie, Bd. 25, p. 431.

portant deficiency of this theory is its failure to explain the acceleration of the pulse after section of the vagus.

Von Bezold's * statement is, with a slight addition, as little open to objection as any. The heart possesses three motor sytems. The first is in the heart itself. The second, the sympathetic, accelerates the pulsations, not because it is the excito-motory nerve of heart, but because it causes greater irritability and diminishes the conduction of resistance. The regulatory system or vagus removes not only activity of musculo-motor nerves of heart, but the action of the sympathetic. To this it should be added, that it requires a large or moderate amount of stimulus to act in this manner, and that slight irritations act in the same direction as the sympathetic.

The true theory of the moderating action of pneumogastric, when found, will probably explain also the action of the chorda tympani on the vessels of the submaxillary gland, and of the nervi erigentes on the vessels of the erectile tissue of penis, and possibly some of the other phenomena connected with the heterogeneous group of restraint nerves.

Many of these phenomena are due to excitations so violent as to approach the pathological in character. Such are for our present case the strong intermittent current (the constant not producing stoppage of heart), and the abdominal irritation in the experiment of Goltz. Probably some cases of sudden death from various kinds of shock are of the same character.†

* Verhandlungen der Academie zu Berlin.

† Phys. and Path. of Nervous Centres, Brown-Séquard, p. 159.

SECTION VII.

THE best point of departure for the study of the universally acknowledged influence of the nervous system on the secretions, is the well-known experiment of Bernard on the submaxillary gland of the dog.

This consisted in exposing the gland with its excretory duct, into which a tube was inserted. Then the artery and vein, or veins, were dissected out, together with the sympathetic filaments accompanying the former, and the branch of the lingual (lower division of 5th) nerve, which goes to the submaxillary ganglion.

The last-mentioned branch is found to be really the chorda tympani, which, leaving the facial, and passing through the tympanum, is attached for a short distance to the lingual nerve, to be again separated as the submaxillary branch. This nerve unites with the sympathetic filaments at the ganglion, which sends branches to the submaxillary gland.

Leaving aside for the present the action of the ganglion as a reflex centre, the two nerves described are found to have actions nearly opposed to each other upon the secretion of the gland. The chorda tympani, whether excited by itself near the gland, with the lingual, or even with the facial, makes the secretion more abundant.

This has been observed by several experimenters under different conditions.

As performed by Bernard, the experiment showed the action upon the circulation as well as upon the secretion. The blood, on stimulation of this nerve, passed more rapidly through the gland; the color was bright, and it differed but little from arterial blood. Sometimes the pulsation was continued from the arteries through into the veins.

When, on the contrary, the sympathetic filaments were excited, the flow of blood became less, and sometimes almost ceased; the color became dark, and the secretion, instead of

a rapid flow of watery fluid, either ceased, or was reduced to a few drops of very viscid saliva.

The section of one of these nerves acted to a certain extent in the same direction as the excitation of the other; and when, in addition, the opposing nerve was galvanized, the effect reached its maximum.

When the gland is in repose, that is, when all the nerves are uninjured, and no stimulus is applied to the mucous membrane of the mouth, the blood which returns by the veins is dark.* If the sympathetic is cut, it becomes much lighter colored. If the chorda tympani is excited, it becomes lighter colored, and runs through the gland much more rapidly, so that the same quantity traverses the gland in one-fourth the time.†

The blood which passes so rapidly through the gland holds a place, as regards its chemical composition, between the arterial and the dark venous. An analysis of the red blood of the renal vein, which is probably analogous to that which comes from the submaxillary gland during galvanization of chorda tympani, showed that it contained nearly as much oxygen as the arterial blood.‡

Bernard also thinks that the redness of the blood which comes from the head, especially of the horse, after section of sympathetic, is partly due to a degree of cutaneous respiration, which in the horse is much more active than in many other animals.

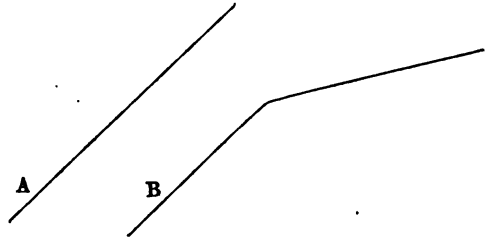
The physiological stimulation of tongue by vinegar or sapid substances ceases to produce an effect upon the secretion of the gland when the tympanico-lingual nerve is cut above the branch to gland; but, even after this, the ganglion acts for a time as a reflex centre for certain other stimuli, which seem to be those which produce dryness or cooling of the membrane. The galvanization of the nerve also produces secretion.§

Experiments somewhat similar to these have been made

* Bernard, Liquides de l'Organisme, II., p. 270. † Ibid., p. 272.
‡ Bernard, Liquides, II., p. 535.
§ Bernard, Comptes Rendus, 1862, p. 346.

by Czermak,* who, however, excited both the nerves at the same time. He applied a tube to the duct, which enabled him to see the rate at which the saliva was secreted, and illustrated the results by a diagram.

He found that excitation of lingual nerve produced, with rare exceptions, rapid and continuous secretion. If, while this secretion was going on, the sympathetic was galvanized, the secretion was much reduced, and the reduction lasted for some time after the cessation of the sympathetic irritation. His diagrams are roughly imitated in the subjoined lines:—



A being the secretion while the lingual nerve alone was excited, and the angle in B representing the point at which the sympathetic irritation began.

He further found that irritation of the sympathetic in neck may produce salivary secretion in submaxillary gland.

The rise of the column (of saliva in the tube) is slight, and lasts only for a short time, notwithstanding continuous irritation, but begins again of itself after interruption of irritation. In only one instance did sympathetic produce continuous secretion like irritation of nervus lingualis.

Similar experiments, though not so complete as those of Bernard, had been previously performed by Ludwig.

The interpretation which was naturally given to these facts, and which subsequent experiments have been undertaken to support or to falsify, was to attribute all the effects to the influence of the nerves on the circulation. The sympathetic is well known to contract the blood-vessels, and would render

* Sitzungsbericht der Akademie zu Wien.

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this secre... ranch of the facial, joined to the auriculo-temporal.*
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...ittich's ‡ experiments, however, are the most entirely
...ctory to all the others. According to him, galvaniza-
...the sympathetic causes a copious and continuous secre-
...thin saliva from the parotid duct. His experiments
...n sheep, dogs, rabbits, and cats. He observed, during the
...ization, dilatation of pupil and contraction of vessels of
...and not only did the secretion continue during the anæmia,
...o contraction of vessels, but also after ligature of portal
..., producing anæmia of upper part of the body. Dur-
...poisoning by woorara the vaso-motor function of the sym-
...hetic lasted longer than the secretory. That is, its galvaniza-
...n caused contraction of the vessels after it had ceased to
...ovoke a farther rise of fluid in the tube connected with the
...arotid duct.

All the facts of secretion in anæmic glands are difficult, and those of Von Wittich are impossible to explain at present on any theory of circulation consistent with the ordinary views; but strongly as they may point to a direct action of the nerves on the secreting cells, yet there are other facts which show that although secretion is greatly influenced by the nerves, it is not dependent upon them for its continuance.

Secretion is something more than a mere filtration of its constituents from the blood through the cells to the duct. This is

* Journal de la Physiologie, IV., 654.
† Sitzungsbericht der Wiener Akademie, 1860, 526.
‡ Virchow's Archiv, Bd. XXXVII., p. 93.

shown by the increased pressure * and increased heat † in the duct above those of the artery.

A strong impulse has been given to the direct secretory action theory, by the alleged discovery of Pflüger ‡ that the nerves which enter the submaxillary glands are intimately united with the cells, in some cases having been seen to enter them and connect themselves with the nucleus. He describes, also, a connection of many fine varicose fibrils with the cells lining the "salivary tubes."

His observations are partially confirmed by Boll,§ who has seen the nerve-tubes enter the alveoli of lachrymal glands of sheep, and submaxillary glands of rabbits, and also a branching of nerve-fibres among the cells. He does not, however, describe any connection with the nuclei of cells.

In opposition to the theory supported by these observations, we can adduce the fact that a destruction, so far as can be known, complete, of all nervous communication does not prevent a gland from secreting.

Bidder ‖ states that cutting nerves of submaxillary gland does not stop secretion, but reduces it to a minimum, to a clear, aqueous, alkaline liquid, which continues to run several weeks. Nevertheless the gland atrophies considerably.

Bernard ¶ says, after cutting tympanico-lingual nerve the gland secretes continuously, but after three weeks, in one experiment, was no longer to be excited by irritation to mouth. The gland on side where this had been done was entirely insensible to galvanization of any portion of cerebro-spinal nerve and ganglion. On examination the gland was not altered.

Eckhard's *[1] results seem to differ materially from those of Von Wittich. He found that the saliva is secreted continuously in sheep, and is uninfluenced by nerves. He stimulated successively, by electrical current, the branches of fifth, seventh, and sympathetic nerves, without change; nor did section of

* Ludwig, quoted by Bernard, Syst. Nerv., II., p. 164.

† Ludwig and Spiess, Sitzungsbericht der Wiener Akademie.

‡ Die Endigungen der Absonderungsnerven in den Speicheldrüsen. Bonn, 1867.

§ Quart. Jour. Micr. Science, Oct., 1868.

‖ Archiv f. Anat. Phys., &c., in Journal de l'Anatomie et de la Physiologie, 1, 1868, p. 110.

¶ Comptes Rendus, 1862, p. 346.

*[1] Brit. and For. Med.-Chirurg. Rev., vol. 40, p. 253.

sympathetic, nor extirpation of cervical ganglion, produce any effect, nor excitation of mucous membrane of mouth.

Pflüger * does not agree, however, with the authors just quoted, but says, "it is perfectly true that, without circulation of blood, a flow of saliva can be excited by simple irritation of the nerves; but that without excitement of the latter, even with complete circulation of the blood, no secretion can be obtained for hours." "If I, in this experiment, suddenly tetanized the very carefully prepared glandular nerve, just as suddenly the saliva shot forth in a stream."

These experiments on the salivary, and especially the submaxillary glands, being, from the accessibility of the parts, by far the most complete and decisive which have been made, it is mainly upon them that we must depend to discover the extent and nature of the nervous influence on secretion. It is impossible to reconcile all the conflicting results, involving, as some of them do, direct contradictions, and implying others, unless we resort to hypotheses of different physiologies for different glands and different animals.

A few general principles, however, may be regarded as well established, although all the observations cannot be harmonized even with these.

The glands are not dependent on the nerves for their secretory power. Besides the experiments above quoted, which are only contradicted by the single assertion of Pflüger, there are many analogies which show the power of a single cell to elaborate organic products. Such are the growth of the ovum in its early stages, and that of the lower forms of animal and all of vegetable life.

Experiments which shall be conclusive on this point are very difficult, especially since Von Wittich's results would make it necessary that all sympathetic filaments should be divided, an operation almost impossible in a gland of any size. Eckhard † found that section of inguinal and lumbar nerves had but little influence on the secretion of milk in goats. This experiment, however, is open to the objection just stated.

Besides the power which the glands have of acting to a cer-

* Op. cit., p. 6. † Bernard, Liquides, II., p. 229.

tain, probably quite limited, extent independently, the nervous system has a great influence on their action, and this influence may be either to excite or to restrain the secretion. It is known to be exerted through the circulation, and supposed to be also transmitted directly to the secreting cells. The first experiment described in this section, the most complete which has been performed, exemplifies the first method so plainly that no further description is necessary. The action of the sympathetic and its opponent on the circulation is in accordance with many other experimental facts.

Other secretions which take place more rapidly after section of sympathetic are the cutaneous and mucous, and it is well known that the secretion is in these cases an accompaniment, and undoubtedly a consequence of a more active circulation. Brown-Séquard * showed that in a Guinea pig which had been impregnated a short time after section of spinal cord, the mamma of paralyzed side secreted milk more abundantly than the well side.

The circumstances which are difficult to account for, as dependent on the circulation, are the occurrence of a few drops of viscid saliva secreted under influence of sympathetic (*Bernard*, *Ludwig*, *Czermak*), the few drops secreted from parotid of a decapitated animal under irritation of facial, or from conjunctiva under irritation of trigeminus (*Czermak*), and especially the continuous and abundant secretion of the parotid under sympathetic influence (*Von Wittich*).

It is true that if all these data harmonized among themselves they would speak very strongly in favor of the existence of a direct action by the nerves upon the cells, especially when supported by anatomical evidence.

Von Wittich's data are, however, entirely exceptional. They agree neither with the effects of galvanization of other nerves in other glands, nor with the effects of sympathetic irritation in the submaxillary gland, or elsewhere in the body. They are, besides being unsupported by any other experimenter, directly contradicted by Eckhard. † As to the few drops of saliva which

* Boston Med. and Surg. Journal, Feb. 18, 1869.

† Since consulting Eckhard's original paper, which was not accessible at the time the above was written, I find the contradiction of facts not so direct as

have been often observed both after galvanization of sympathetic, when the circulation was otherwise undisturbed, and after galvanization of either nerve in an anæmic gland, it is admitted by Pflüger as possible, though not probable, that they may be pressed out of the gland and not secreted at the time. This, however, is not agreed to by Czermak.

Whether a derivative circulation like that described by Sucquet * in the limbs and face, and which is said by Bernard † to exist in the glands, being possibly controlled by a different set of nerves from the nutritive circulation, has anything to do with the variety of these phenomena, is for the present unknown.

While, therefore, we cannot, being at present unable to account otherwise for all the observed phenomena, deny positively the existence of any direct secretory action, yet we are by no means inclined to regard it as conclusively proved. The experiments quoted would also be impossible to reconcile as to which nerve had this influence, if it existed. Some speak as strongly for the cerebral motor nerves as others for the sympathetic. No data comparable to those from the salivary glands have been obtained from other organs.

After section of the renal nerves, the kidney is decomposed with great rapidity, and putrid material passes into the circulation, so that the animal dies.

The liver furnishes rather more valuable results in regard to one of its secretions; the mechanism of the other not being so clear, although pathology shows that it is not entirely removed from the influence of the nervous system.

The formation of sugar by the liver, although not a strictly physiological process, illustrates very well the two stages of secretion. The glycogenic material, the animal starch, is a product of vital forces—that is, of a chemistry which takes place only in the living body, and through the agency of cells. This is, if our theories are correct, a slow and nearly uniform process, the organ depending on the blood, of course, for its material, but elaborating its peculiar product without being urged by the pressure in the vessels, or by nervous stimulus.

I had supposed. His interpretation, however, is entirely different from that of Von Wittich.

* Comptes Rendus, 1866, II., p. 525. † Liquides, II., 161.

The transformation of glycogen into sugar, and its passage into the blood, is the second stage, and is dependent on the circulation, and through it upon the nervous system. The production of an artificial diabetes by puncture of the floor of the fourth ventricle is a well-known experiment.

"The puncture of the middle of the space between the origin of the pneumogastrics and that of the auditory nerves determines at the same time an augmentation of the quantity of the urine, and the appearance of sugar therein." "Puncturing a little higher, the urine is less abundant, less loaded with sugar, but frequently contains albumen." "Puncturing the medulla oblongata a little below the origin of the auditory nerves, there is an increase of the quantity of urine, without the passage of either sugar or albumen."*

Other lesions, such as section of a peduncle of cerebellum, or of the cerebrum in front of the peduncles, may also cause diabetes.†

Section of the pneumogastric does not prevent this effect from taking place, nor that of cervical sympathetic; but section of spinal cord does, even when made so low down as second dorsal vertebra, but not at the level of the first lumbar.‡

Experiments on frogs have shown that if the supply of blood is cut off by ligature of vessels supplying the liver, puncture of the floor of ventricle ceases to produce diabetes. On removing the ligature the process goes on as usual.§

It is probable that the vaso-motor fibres arising from the encephalic centre, and controlling the abdominal circulation, pass through the region which, when punctured, causes diabetes. When the puncture is made, these fibres are either cut off, or, what is more probable, temporarily paralyzed by the pressure or violence, thus permitting a neuro-paralytic hyperæmia of the corresponding organs. The various organs are also capable of being separately affected, as is shown by carrying the puncture lower down than the auditory nerves, and thus paralyzing only the renal vessels, whence the increased flow of non-saccharine urine. At the higher point both these fibres,

* Bernard, Système Nerveux, I., p. 398.
† Ibid., pp. 440, 441.
‡ Ibid., p. 446.
§ Ibid., 464.

and also those controlling the hepatic circulation, are paralyzed. What organ throws albumen into the circulation is not known.

These fibres pass down through the spinal cord, leaving it probably in the dorsal region, and traversing the splanchnic nerves. Section of these nerves has been seen to be followed by diabetes.*

It might seem that on this theory every section of the spinal cord in the cervical region ought to produce diabetes, instead of preventing its occurrence after puncture, as it was found by Bernard to do.

This seeming anomaly is probably due to the increased reflex activity of the cord which results from its section in the cervical region; an activity which is not fully accounted for, but which has been explained by supposing it to be cut off from a centre situated in the brain, which restrains reflex action (Setchenow †), or which might possibly be caused by a hyperæmia of the cord itself, vaso-motor fibres distributed to its own nutrient vessels having been severed.

Schiff, ‡ however, says that diabetes may be produced by section of the cord, and distinguishes a diabetes due to irritation as well as one due to paralysis.

If the interval between sixth and seventh cervical vertebræ is laid bare, and posterior strands of cord destroyed, an abundant secretion of sugar occurs, lasting five or six hours.

"When this diabetes from irritation has subsided, any further destruction of posterior strands remains always without effect; but if the rest of cord is cut through, the diabetes of paralysis is set up, which continues unaltered until death."

So far as the action on the liver is concerned, both kinds of diabetes are probably the same; the former depending on a reflex paralysis of vaso-motor nerves, the latter on a paralysis from section.

Diabetes has also been produced by drugs; the action on the hepatic circulation being probably similar to that produced by lesion of medulla oblongata.

* Several authors, quoted by Eulenburg and Landois. Wien. Med. Wochenschrift, 1867, No. 97.

† Vulpian, op. cit, p. 438.

‡ Brit. and For. Med.-Chi. Rev., Oct., 1862, p. 351.

Saikowsky * obtained the most persistent diabetes which has been observed as a result of experiment, by injecting subcutaneously, and sometimes administering by the mouth, various preparations of mercury. He found at the autopsies of the rabbits used, very considerable dilatation of vessels, especially of the liver and kidneys. In two animals the diabetes lasted at least four weeks after they had taken, for from thirteen to eighteen days, two grains daily of corrosive sublimate.

Dr. Harley † rendered urine saccharine by injection into branches of portal veins of ether, chloroform, liq. ammoniæ, and methylated spirit.

As the essential condition in producing an increased secretion of sugar by the liver seems to be an increased supply of blood, the point most difficult to explain is why, when the portal circulation must be continually sending a not inconsiderable current through the liver, and during digestion undoubtedly increasing the amount, the urine is not saccharine more frequently.

I have only to make a suggestion that the ferment necessary to convert the glycogen into sugar is contained in the blood of the hepatic artery, and not of the portal vein, and that the vaso-motor paralysis affects the branches of the former vessel, which in a normal condition transmits but little blood.

It is well known that the quantity of ferment, whatever it may be, necessary to change glycogen into sugar is extremely small. Even if a piece of liver cut out and thrown into boiling water is too large to be immediately heated through, sugar will be formed in its interior, so that it is hardly possible, if the ferment were contained in the portal blood, that a large amount of glycogen should not be continually undergoing the transformation into sugar—a transformation which the later researches have shown to be abnormal, at least beyond a very small extent.

* Virchow's Archiv., Bd. XXXVII., p. 346.
† Brit. and For. Med.-Chi. Rev., Oct., 1862, p. 351.

SECTION VIII.

THE control which the sympathetic, through its vaso-motor action, has over the excitability and vitality of parts is very well marked and very important.

The increased heat and augmented secretion after section of the sympathetic have already been described, but there are other phenomena, less striking to be sure, which are also dependent on the freer flow of blood, partly from the more abundant supply of nutriment, and partly from the increased heat.

Nervous and muscular irritability are increased, as is shown by the contraction of muscles at the angle of the mouth, at the nostril, and the ear.

Brown-Séquard* also found greater sensibility to strychnia and the galvanic current, as well as a longer continuance of contractility after death, on the side where the sympathetic had been cut.

Liegeois† considers the exorbitis produced in frogs by hemisection of bulb, to be due to increased excitability of muscles, especially the most vascular, which is that which forces eye out of socket. He shows that this is due to the vaso-motor action, by removing the heart and restraining the animal from voluntary movements, in which case the exorbitis does not take place.

The contraction of the pupil and the protrusion of third eyelid, observed after section of the sympathetic in the cat, are probably partly due to an increased sensitiveness of the eye to light, as they become more marked as the light is stronger, and as they show a tendency to supplement each other's action; the contraction of pupil, for instance, being greater after excision of a portion of third lid.

Brown-Séquard,‡ in commenting on the explanation by

* Phys. and Path. of Nervous Centres, p. 145.

† Journal de l'Anatomie et de la Physiologie, 1864, p. 62.

‡ Journal de la Physiologie, I., 429.

Landouzy (de Rheims) of the increase of auditive sensation in facial paralysis as a paralysis of internal muscle of malleus, says that extirpation of superior cervical ganglion, or section of sympathetic near ganglion, has the same effect, which is, however, due to paralysis of vessels of auditory nerve, making it more sensitive.

The nervous and muscular excitability of a posterior limb, after section of spinal cord on the same side, are increased in the same way in which they are found to be after section of sympathetic.*

The opposite condition of the sympathetic, that of irritation, does not carry with it such marked consequences. Of course the phenomena due to increased circulation are not seen, but loss of muscular irritability is not an immediate consequence of loss of blood. This is seen in the convulsions following bleeding, when the muscles must of course receive less than their normal supply.

The theory of the loss of excitability from spasm of blood-vessels has been carried very far by Brown-Séquard, who explains thereby the stoppage of the heart on galvanization of the pneumogastric, and the restraining influence of the splanchnic nerves on the peristaltic movements, as well as reflex paraplegia.

The contraction of vessels in the spinal cord has been doubted, as will be seen in the section of pathology treating of reflex paralysis, and it is well known that both the heart and the intestines continue to execute movements when entirely removed from the body, and, of course, nearly destitute of blood, or at least incapable of renewing it. A contraction of blood-vessels, then, can hardly account for the immediate stoppage of the heart's action which ensues upon galvanizing the pneumogastric.

Panum† has confirmed the fact of contraction of coronary arteries under the influence of an irritation of the pneumogastric; but the complete arrest of the heart always manifested itself in his experiments before the diminution of calibre of these arteries had reached its maximum.

More marked phenomena following sympathetic irritation are those which take place from diminishing suddenly the supply of blood to the encephalon.

* Brown-Séquard, Phys. and Path. of Nerv. Cent., p. 145.

† Quoted by Samuel, Jour. de la Phys., III., 572.

Kussmaul and Tenner * found that after ligature of both subclavians (to cut off vertebrals) and one carotid, galvanization of sympathetic on opposite side produced convulsions.

The loss of consciousness in epilepsy may easily be supposed the consequence of cutting off the supply of blood by vascular spasm, and the foregoing experiment would render it more probable that the convulsions are produced directly by anæmia than that they are the result of a secondary accumulation of dark blood at the base of the brain.

The effects of a more permanent vaso-motor paralysis may vary somewhat, though not within wide limits. Perhaps the reason why extreme pathological effects are so seldom seen, is the infrequency with which a complete vaso-motor paralysis is produced. The ordinary section of the sympathetic in the neck is followed by no consequences in regard to nutrition, but the increased irritability described above, and a greater liability to inflammation. The increased mucous secretion consequent upon this operation may, if the animal is sickly, become a purulent one.† The liability to inflammation, however, is in this case hardly a disadvantage, since it ends, as well as begins, with greater promptness.

The hypertrophy which results from section of nerves is very limited in amount, and confined almost entirely to epidermoidal tissues, producing a roughness of skin and curvature of nails. Even these changes cannot always be attributed clearly to paralysis of vaso-motor fibres. A general hypertrophy has never been observed. The cases of pathological unilateral hypertrophy have prompted experiments on partly grown animals, to determine whether the increased supply of blood consequent on section of sympathetic would determine an increased growth of tissue. Ollier ‡ found that the ears of young rabbits thus operated upon were of the same length as those of the other side after three months and a half.

Among all the animals experimented upon in this way by many physiologists, there must have been a large number which had not reached their growth. No report of hypertrophy has been made concerning any of them.

* New Sydenham Society's Translation. † Bernard.

‡ Journal de la Physiologie, VI., p. 106.

There are but few experimental data as to a permanent irritation of a nerve, and its effect on the parts to which it is distributed. The experiments of Samuel, with the counter-experiments of Tobias and Weber, will be spoken of more fully in the pathological portion. There was no atrophy observed.

The case of unilateral facial atrophy, which is also given in that portion, was possibly due to irritation of cervical sympathetic, but this is a mere hypothesis.

The ulcerations and sloughing due to irritation of nerves are also spoken of in the pathological sections.

The question of the existence of trophic nerves, or nerves having a direct influence on cells other than muscular, though not included in our subject, is closely connected therewith in regard to nutrition in general, as well as in regard to secretion.

Besides the facts adduced in connection with secretion, some very interesting observations of Mr. Lister * may be mentioned, which, though not conclusive, tend to favor the theory of a direct action. He found the color of frogs to depend on the distribution of pigment granules within the stellate pigment cells, and that this color might change by a concentration of the granules toward the centre of the cell or distribution into the branching processes. The influence of the light upon this color was through the eyes, and not directly upon the skin. The struggles of the animal produced concentration of the pigment and consequent paleness of surface.

It has been attempted throughout this essay to account for all action of centrifugally conducting nerves on the nutrition of the tissues, without supposing any connection with other than contractile or muscular fibre, or with another nerve.

Although there are facts, as will especially be seen in the pathological portion, which are difficult to account for by supposing vascular changes always to be concerned in the action of the nerves upon the other tissues, excepting, of course, muscles and the terminations of sensitive nerves, yet from their not very great number, and the inconclusiveness of any evidence which has yet been brought forward in support of the direct "trophic" action, it is more philosophical, for the present, to regard the existence of such action as not proven.

* Philosophical Transactions, 1858, p. 627.

SECTION IX.

THE branches of the sympathetic distributed to many of the abdominal and pelvic viscera perform a twofold office. For all the secreting organs they regulate the supply of blood and control their activity, while for the intestines and genito-urinary apparatus, and, to a smaller extent, probably the spleen, excretory ducts of glands, and the muscular apparatus of erectile tissue, they supply the stimulus to peristaltic action, or simple contraction.

The power of the intestines to perform regular peristaltic movements when separated from the body, is probably due to the two plexuses contained in their walls; that of Auerbach seeming, from its position between the two muscular layers, to be the special agent of the general and powerful muscular action, while that of Meissner is more closely connected with the submucous muscular layer, and probably the circulatory and secreting apparatus.

It has been proposed to explain this action by the stimulus which one muscular fibre in contraction has upon that next to it, and so on. The fact that the movement is always propagated downward,* seems, however, to demand some more complicated arrangement than that of mere contiguity, in which the irritation should spread in both directions equally.

The account of the experiment of Müller, quoted by Todd and Bowman,† in which caustic potash was the irritant used, does not state whether any section of splanchnic nerve was made above semilunar ganglion; but it is fair to suppose from the negative evidence that the nerve was intact, thus rendering the experiment inconclusive as to the effect of direct stimulus, since the peristaltic motion of the intestines might take place by reflex stimulus from the splanchnic as a sensitive nerve, just as vascular contraction in the same region takes place from irritation of either extremity of the same nerve divide

* Bernard, Sys. Nerv., I., 361.

† Physiological Anatomy and Phys. of Man, p. 508.

The same objections apply to the experiments of Onimus (loc. cit.) on the action of the direct current.

Kupffer and Ludwig,* however, galvanized the divided splanchnics with great precautions, to preserve, so far as possible, the neighboring parts from being affected by the current. They found that almost constantly, soon after death, the excitation produced peristaltic movement. They experimented on cats, because the intestines of these animals do not, as in rabbits, take on automatic movements on exposure to the air. The vagi, when galvanized in the neck, were also able to excite the movements of the intestines. It is remarked of both the nerves that they do not affect the whole intestine, but sometimes one part and sometimes another.

The splanchnics have been considered the restraint nerves of the intestine. Pflüger † found that galvanization of a certain portion of the spinal cord, or of the great splanchnics, arrests movements of the small intestines and leaves them in a passive condition. The lesser splanchnics have the same effect, but neither seem to act on large intestine.

Of the occurrence of this action there is no doubt. It has been confirmed by others, but the interpretation is nearly allied to that of the action of pneumogastrics on the heart, in being greatly dependent on the amount of the irritation. Kupffer and Ludwig, in the experiments just described, observed both the stimulating and restraining action of the splanchnics. The splanchnics begin to lose their restraining power at about the time the vagi are acquiring their stimulating properties.

It is also true in this case, as in regard to the pneumogastric, that the restraining action may take place from other stimuli than those applied to the nerves themselves. Mr. Lister found that sometimes a struggle on the part of the animal, after the intestines were in movement, was followed by their complete quiescence.

Mr. Lister ‡ says very judiciously, "that it is safest, in the present state of science, to regard as a fundamental truth not yet explained, that one and the same afferent nerve may, accord-

* Sitzungsbericht der Wiener Akademie, 25, p. 580.

† Review in Journal de la Physiologie, I., p. 421.

‡ Proceedings Royal Society, vol. ix., p. 367.

ing as it is operating mildly or energetically, either exalt or depress the functions of the nervous centre on which it acts." The nervous force in the pneumogastric, splanchnic, chorda tympani, nervi erigentes, and of course all sensitive nerves, passes through ganglionic centres before it reaches the final distribution of the nerves to the contractile or possibly secreting elements. "It is, I believe, upon this that all inhibitory influence depends, and I suspect that the principle will be found to admit a very general application in Physiology."

The spinal centres for the peristaltic movements are probably the same as, or very near to, those which control the intestinal circulation, and correspond to the places of exit of the roots of the splanchnic nerves. The connections of the nerves with each other, and with the ganglionic centres, are such that an irritation is not carried back as a motor impulse to the part whence the irritation proceeded, but lower down.

Irritation of the first thoracic ganglion, or the superior extremity of a section of sympathetic made below this ganglion, produces movements of the small intestine. Galvanization of the solar plexus produces peristaltic movements of large intestine. An irritation of the nerves of the small intestine makes the large intestine contract, and the nerves of the stomach have the same effect on the small intestine.*

* Bernard, Syst. Nerv., I., 359.

SECTION X.

THE sympathetic is distributed as a motor nerve to several other abdominal and pelvic organs. and the contractions of many of them show the same peristaltic character as those of the intestines.

"The lumbar and sacral portions of the gangliated cord act on the large and small intestine, rectum, ureters, bladder, vasodeferentia, vesiculæ seminales, fallopian tubes, and uterus." * Certain glandular excretory ducts in birds have been seen by Brown-Séquard † to have peristaltic movements.

The uterus is, according to Körner, ‡ supplied with nerves from two sources, either of which excites contraction, and neither of which acts as an inhibitory nerve. The cerebro-spinal nerves, which are branches of the sacral plexus, take their origin from the neighborhood of the third and fourth lumbar vertebræ. The uterine fibres of the sympathetic, coming from the aortic plexus, take their spinal origin at the level of the last dorsal vertebra. Körner also found, with Heidenhain, that the uterus contains, contrary to the statement of Kölliker, a large number of ganglia, mostly confined to the lower part.

The spinal cord at any part was found capable of exciting, on irritation, the contractions of the uterus, and also of the bladder. The movements were, however, more easily produced the more nearly the middle of the lumbar spinal cord was approached.

Experiments made to discover the encephalic centre for uterine movements led to no decisive result, since the irritation of many different portions of the brain alike produced, with the exception of the anterior cerebral lobes, the usual contraction.

These results throw much doubt on the value and completeness of the experiments of Budge § on the production of vesical

* Valentin, op. cit.

† Jour. de la Physiologie, II., 775.

‡ Studien des Physiologischen Instituts zu Breslau, 1865, p. 1.

§ Comptes Rendus, 1864, p. 529.

contraction by irritation of cerebral peduncles, as well as those before mentioned on the vaso-motor centre.

The smooth muscles of the erectile tissue belonging to the genital apparatus, are probably supplied with nerves from the plexus cavernosus, or inferior hypogastric plexus.

Of the influence of the sympathetic, as distinct from that of the pneumogastric, on the smooth muscles of the bronchi, we know nothing, physiologically or pathologically.

SECTION XI.

THE sympathetic is distributed to both constrictor and dilatator of the pupil, and assists in the movements of the iris.

It excites several smooth muscles of the orbit and the skin. It furnishes motor nerves to the heart, and controls the circulation in the blood-vessels, not only regulating the supply of blood to different organs, but, by maintaining a due degree of contraction of all the vessels, exercising an important influence on the general circulation.

The calibre of the vessels is determined by the contraction of the muscular fibres under the control of the sympathetic on the one hand, and the dilating pressure of the blood on the other.

Heat, secretion, and nutrition are indirectly influenced by this system. The direct action either of this system or of the cerebro-spinal nerves strictly speaking, on secreting cells must be regarded as still doubtful.

It is a motor nerve for the intestines and genito-urinary apparatus.

It also contains sensitive nerves, but the sensations usually become reflected motor impulses before they reach the brain. The distribution of the sympathetic takes place almost universally through plexuses containing ganglia, and the peripheral ganglia are able to continue the function of the nerve independently for a greater or less time. It is, in these ganglia, often united with cerebro-spinal nerves, which, under a greater or less degree of stimulation, act as opponents or paralysants, and destroy or diminish its action. They are then called (as in one instance a branch of the sympathetic itself) restraining, or inhibitory nerves. A similar action sometimes takes place in more central ganglia, when a sensitive nerve becomes inhibitory.

This system must be regarded as a dependence of the cerebro-spinal, differing from the other nerves more in its distribution than in its origin. Besides the fibres originating in the cerebro-spinal axis, it includes others which are contained exclusively

in its own territory, but the effect of these cannot be separated from that of the former.

Its motor fibres are physiologically stimulated by reflex irritations, either from its own sensitive fibres, or from those of the remainder of the cerebro-spinal system. It is also affected by psychical stimuli.

The reflection may take place in the ganglia near the distribution; in those nearer the centres, probably; in the spinal cord; or in the encephalon.

The course of the sympathetic motor fibres in the spinal cord is probably in the anterior columns.

The encephalic centres for the different classes of sympathetic fibres are as yet unknown. They are probably above the medulla oblongata.

Several portions of the spinal cord from which motor fibres for special organs or groups of organs arise, and from which stimuli act on those organs with greatest intensity, have been called spinal centres, and received names corresponding to the organs influenced.

PATHOLOGY.

ANATOMY.

If our list of diseases or affections of the sympathetic system were to include only those proved to be such, it would be a very short one; but if, on the contrary, it were to consist of all that have been supposed to be caused by troubles of this system, it would be very much like a treatise on the theory and practice of medicine.

I shall try to discriminate between those diseases in which there is some proof, either from pathological anatomy, experiment, or very strong analogy, that they involve, as an essential part, some affection of the sympathetic system, and those where there is but little beyond conjecture; but, as I shall endeavor to state fairly what the nature of the proof is, my statements cannot greatly mislead, even if my judgment should be in error.

The forms of structural alteration, affecting the cords and ganglia of the sympathetic, are few in number; and the recorded cases in which lesions have been observed are also very few.

Longet * says that Bichat had never seen any lesion of sympathetic, except in one case of cancer of the stomach, where a semilunar ganglion was enlarged and its density a little increased.

M. Charles Robin observed a tumor of solar plexus, formed of nervous cords ramified and anastomosed. The cords were enlarged, grayish, semitransparent, a little gelatiniform at centre, composed largely of connective tissue and amorphous material. In the masses corresponding to ganglia were ganglionic cells similar to the normal. Verneuil reported to the Surgical Society a case of cancer beginning in superior cervical ganglion. A hard,

* Anat. et Phys. du Système Nerveux, ii., p. 625.

homogeneous, lardaceous tumor was reported in Müller's Archiv, 1838.*

Lobstein, quoted by Longet,† saw in a young woman who died in the fifth month of pregnancy, of continual vomiting, the semilunar ganglia red and inflamed, and had also observed a similar lesion in a case of whooping-cough, complicated with frequent spasmodic vomiting.

Rokitansky ‡ describes a withered state of the ganglia, a shrinking and leather-like toughness, while their color has either altogether disappeared, or is changed to a rusty brownish, yeast yellow, fawn, or slate gray.

The chief example of it is in the wasting of the abdominal ganglia which follows typhus.

* All the cases in this paragraph are from Polaillon. Journal de l'Anatomie et de la Physiologie, 1866.

† Op. cit.

‡ Pathological Anatomy, Syd. Soc. Trans., vol. iii., p. 459.

SECTION I.

THE sensitive fibres of the sympathetic must, of course, be the path followed by many painful sensations originating in the viscera, but it would not be within the province of this essay to describe all the diseases which might give rise to them.

Pure neuralgias of the sympathetic must be very difficult to identify as such.

Romberg,* however, describes several diseases under this head. There may certainly be a good deal of doubt of the propriety of thus classifying angina pectoris; first, because the pneumogastric as well as the sympathetic enters into the formation of the cardiac plexuses; and, second, because it is usually associated with some organic lesion.

Cœliac neuralgia is a violent contracting pain at the pit of the stomach, sudden, or preceded by a sense of oppression. It generally extends to the back, there is a sense of fainting, the face is fallen in, the hands and feet cold, and pulse small, cramped, and intermittent. The peculiar character of the pain, sense of fainting, and vascular affections are regarded by Romberg as pathognomonic of cœliac neuralgia.

Mesenteric neuralgia is colic. Romberg says "that anatomical research has not up to the present time demonstrated any structural lesions in the ganglia or plexuses of the sympathetic," except that Dr. Segond, whose authority Romberg seems to doubt, communicates instances of hypertrophy, alteration in color, and induration. In lead colic, Tanquerel found hypertrophy of ventral ganglia of sympathetic.

There may be some force in objecting to the usual pathology of colic, that a spasm is not likely to last so long as lead colic does; but certainly a similar course of reasoning would go far to disprove the theory here advanced of ordinary colic, for any one who has had experience of the two affections must have wished that he could see any other neuralgia, facial or sciatic for instance,

* Nervous Diseases of Man. Syd. Soc. Trans.

disappear with the rapidity, and especially with the completeness, that colic does.

There is no doubt about the pain in this affection, and but little as to the existence of spasm; which is the essential characteristic of the disease cannot be decided at present.

SECTION II.

THE iris is not infrequently affected in diseases and injuries involving the nervous system, and although probably in the larger number of cases the cause is to be found in the brain acting through the third and fifth nerves, yet there is a not inconsiderable proportion where the pupil furnishes symptoms of lesions in the course of the cervical or upper thoracic cord of sympathetic, its rami communicantes, or its spinal centre.

The most frequent lesions are tumors of the thorax or lower cervical region, and they may act in either of two ways: first, when their pressure is great, causing paralysis; and, secondly, when the pressure is less, causing irritation; contraction being the symptom of the first condition, and dilatation of the second.

Flint * and Aitken † mention contraction of the pupil as a symptom of thoracic aneurism, and Da Costa ‡ also speaks of dilatation as a more rare event.

Ogle § has collected a considerable number of cases in which these symptoms occurred. Besides several cases of contraction of the pupil on the same side with a thoracic aneurism, he mentions one, in which, together with this symptom, there was present during the patient's recovery a remarkable irregularity of temperature; cold sweats followed by flushing, accurately limited to that half of face on which pupil was affected. In another case the contraction of left pupil coincident with carotid aneurism nearly disappeared after ligation of the artery. Some cases of injury to lower cervical and dorsal vertebræ (cilio-spinal region) were followed by contraction of pupils.

Another series of cases consists of those in which dilatation took place on the affected side. These include aneurism, scirrhus, and glandular and inflammatory swellings of neck. The most no-

* Practice of Medicine, 1867, p. 336.
† Science and Practice of Med., Amer. Ed., 1866, p. 782.
‡ Med. Diagnosis, p. 320.
§ Med. Chi. Trans., vol. xli.

ticeable are, one in which a peculiar state of mortification was set up in the side of the nose, corresponding to the side chiefly affected by aneurism; and one in which phlegmonous inflammation in neck, with a tumor, was followed by alternations of contraction and dilatation, while the color of the iris changed from gray to dark brown.

The insidious disease of the nervous centres, especially of the posterior columns of the spinal cord and posterior roots of the nerves, known among other names as progressive locomotor ataxy, has been thought, from its presenting in certain cases symptoms on the part of the iris, to be a disease of the sympathetic.

Duchenne de Boulogne, * whose name is so prominent in connection with this disease, calls attention to the (1) contraction of pupils, and during painful crises, considerable dilatation of them in a woman affected for twenty-three years with ataxy; (2) contraction of pupil with augmentation of vascularity, and calorification of the eye with dilatation of pupil and diminution of vascularity during painful crises, in a subject affected for ten years; (3) contraction of pupil in painful crises in second period, with augmentation of vascularization and heat of eye during the contraction.

Some other observations of the same character are those recorded by Voisin, † dilatation of pupils following pinching, pricking, or irritation by galvanism; and Echeverria, ‡ dilatation of pupil on same side as pinching or pricking of arms or legs. In the latter case the irritation is also followed by perspiration of palm of hand and fingers. In the case recorded by Bazire § the left side of forehead was covered with perspiration during the painful paroxysms, the right being dry.

The foregoing observations show, with others similar, that the oculo-pupillary, and occasionally the vaso-motor fibres of a limited region, are involved in this affection. It, however, by no means follows that they take an essential part in its causation. If some fibres of sympathetic are concerned in a dilatation of

* Gazette Hebdomadaire, 1864, 8, 10, and Comptes Rendus, 1864, p. 168.
† Gazette Hebdomadaire, 1864, 38.
‡ Reflex Paralysis, p. 59, note.
§ Translation of Trousseau's Clin. Med.

vessels and congestion of spinal cord, accompanying or preceding its characteristic degeneration, it is only as they may be in any other nutritive disturbance, probably secondarily, and not in a manner special to this disease.

It is certainly a much less far-fetched supposition that the cause of the pupillary phenomena is simply that the part of the cord where the appropriate fibres take their rise is becoming involved in the lesion, and thus the sympathetic is acted upon from the cilio-spinal centre.

If the sympathetic were the first portion of the nervous system involved, the contraction or dilatation of the pupil should be among the earliest symptoms observed; and as it must be through its vaso-motor fibres, if this theory be true, that the secondary lesions of spinal cord and ganglia are produced, the phenomena of heat and perspiration should be much more constant, and not, as we find them, rather rare and exceptional. The strongest argument against this theory, however, is the almost complete want of anatomical evidence in its favor. Donnezan * has, to be sure, reported one case in which the cervical ganglion was hard and tough, with a peculiar yellowish-white tint. There was no microscopical difference between this and a healthy ganglion, although in ramus communicans the nervous tissue had almost entirely disappeared.

As negative evidence needs to be stronger than positive, I may be allowed to refer somewhat fully to a paper by M. Vulpian,† on four cases of progressive locomotor ataxy, where careful microscopic examinations were made.

I. Female. Disease had lasted probably thirty-three years. Well-marked ataxy of lower limbs.

Sclerosis of posterior columns, the affected portion increasing in width from above downwards. Atrophy of posterior roots. Superior and middle cervical ganglia of left side were normal, and probably the connecting cord, though it was in appearance a little grayish. It contained perhaps less fibres than normal, but no series of fatty granulations. The vessels, both in the cord and the ganglia, were perfectly healthy.

II. Female. Duration fourteen years. Ataxy of legs and

* Gaz. Hebd. 1864, No. 19.

† Archives de Physiologie Normale et Pathologique, No. 1, p. 128.

somewhat of right hand. Painful crises. Spasms. Left pupil more dilated, and sight weaker on this side.

Sclerosis of posterior columns. No abnormal vascularization of superficial or deep parts of marrow. Atrophy of posterior roots. Cervical cords and ganglia of sympathetic apparently normal.

III. Female. Six years' duration. Ataxy of both legs and arms. Diminution of sensibility to touch and pain. Incomplete paralysis of right oculomotorius, and slight paralysis of left abducens. Frequent vomiting. At the end of June, 1867, the patient was almost completely blind. Sensibility to simple contact none, or almost none. Pricking felt as at first. Tickling the feet felt as a simple touch, producing slight reflex movements. Sensibility to cold and heat well preserved. The patient cannot eat without assistance. Her limbs emaciated, very feeble, and movements of hands entirely characteristic (very well-marked ataxy). No albumen nor sugar in the urine. Constant constipation. Vomiting every morning, almost to the last day.

Atheroma and small aneurisms in encephalon. Degeneration of optic and oculomotor nerves. In different parts of encephalon small vessels were altered; and in several places, especially in the cerebellum, some vessels were surrounded by a mould of granular bodies. Considerable injection of superficial vessels of marrow, especially on dorso-lumbar enlargement. Degeneration of posterior columns and posterior roots. Some vessels presented the appearance noted in the encephalon, but many appeared entirely healthy.

A dorsal sympathetic ganglion, removed with the corresponding spinal ganglion, showed a little less pigment than the latter. There were numerous little capillary dilatations observed upon the ganglion. The right semilunar ganglion with its branches and vessels was healthy. The cervical sympathetic on left side was normal, but cellules were also less pigmented than those of spinal ganglia.

IV. Characteristic symptoms for two years and a half. The cervical cords of great sympathetic, and the ganglia with which they are in relation, appeared perfectly healthy to the naked eye and the microscope. The ganglions of the solar plexus examined by the naked eye seemed healthy.

"As in all these observations we are dealing with facts, where the disease was very much advanced, we are, I think, sufficiently authorized to say that the sympathetic does not in general offer any considerable lesion in the case of sclerosis of posterior columns, with atrophy of posterior roots; or at least that these lesions, if they exist, are much less easily appreciable either to the naked eye or by the aid of the microscope, than those of the cerebro-spinal system."

SECTION III.

THE effects of injuries to nerves in man from accident, operation, or disease, correspond, in most cases, where the nature of the lesion can be clearly established, to the results of similar experimental injuries in animals. Beside cases of this class, a vasomotor paresis of cerebral or reflex origin may often be demonstrated from the condition of the circulation.

A few cases partially corresponding to section of the cervical sympathetic have been observed. Such are those of unilateral sweating and flushing, which in some cases are conjoined with a contracted pupil on the same side. There is, so far as I know, no case recorded in which all the phenomena of divided cervical sympathetic, that is, contracted pupils, flushing, sweating, and increased temperature, as measured by thermometer, were noted, and could be referred to organic lesion; but this is undoubtedly owing to defective observation in some particular in the cases observed.

One of Dr. Bartholow's * cases was that of a lady subject to flushing, with profuse perspiration on one side of head; the heat in meatus auditorius being increased by 5°. No special mention is made of the pupil, and the patient was otherwise in good health.

The flushing, together with contracted pupil, supposed to be connected with thoracic aneurism, was observed in a case quoted in a former section from Dr. Ogle.

A case of unilateral flushing and sweating in a man in whom these symptoms had twice been precursors of epileptic attacks, but afterward occurred in connection only with a slight affection of speech, is also reported.†

It is hardly to be doubted that in all these cases there existed a paralysis, complete or partial, of the cervical sympathetic, which controls a large part of the cephalic circulation. This might

* Unilateral Sweating of Head, &c. Quart. Jour. of Psych. Med., Jan., 1869.
† Med. Times and Gazette, April 7, 1866.

have been in the first case either of organic origin, due to some undetected thoracic lesion, or to reflex influence. In the aneurismal cases it was of course due to pressure probably on the inferior cervical ganglion, or its rami communicantes, and in the epileptic case it was as clearly a part of that more or less general disturbance of the nervous system observed in that disease.

The most frequent pathological disturbances to which the vaso-motor system is liable, are the conditions of rigor and fever. There are, however, other considerations which make the explanation of fever by no means simple.

In fever the changes in the blood and in the nutrition influence the circulation and the temperature very largely, and the rise of temperature of the whole body is as much or more a chemical phenomenon than even a vaso-motor one. It is now a well-established fact, that there is in fever a greatly increased tissue metamorphosis, usually measurable in excreta; but whether a vaso-motor paralysis in the whole body, or in the important heat-producing organs in the abdomen, is the principal cause of this, or whether it is chiefly due to the effects of a morbid poison on the tissues, must be a matter of conjecture.

It may be considered that in fever the central temperature undergoes a gradual rise corresponding to the organic combustion, of the exciting cause of which we are ignorant, while the vaso-motor nerves regulate the amount transmitted to the surface.

In a paroxysm of intermittent fever the central heat augments in a much more constant and regular manner than the surface temperature indicates.

The initiatory chill of intermittent fever, as well as of other diseases, is coincident with an irritation of the cutaneous vaso-motor nerves. In many cases of commencing febrile diseases, or even during their course, this action, instead of being a short and decided one, is irregular and weak, giving rise to slight chills alternately with flushes of heat coming and going. With this vaso-motor spasm, restraining the circulation and making the skin pale, comes an irritation of the smooth skin muscles, the arrectores pili, giving rise to gooseflesh.

After this stage comes relaxation of vessels, increased afflux of blood, heat and redness of skin, and throbbing of arteries.

The third stage consists in somewhat diminished activity of the circulation and free secretion.

These stages are observed in ordinary paroxysms of intermittents, or in attacks of febricula. In other diseases they are less marked, or irregular in duration and succession.

The change from the first to the second stage is easily accounted for by the relaxation which usually follows spasm, but the succession of the third to the second is not so simple, especially in regard to the secretions, a point which will be more fully discussed in the next section.

Onimus and Viry,* reasoning from sphygmographic observations, suppose the transition from the first to the second stage to be principally due to the nerve-centres not receiving sufficient stimulus from the limited supply of cold blood sent to them in the first stage to enable them to continue the vaso-motor contraction, and the third stage to depend on their partial recovery from the semi-paralytic state. It is to be remembered, in considering this theory, that the internal temperature is not indicated by that of the skin, and that the nerve-centres receive a supply of warm blood, even while the surface is cold.

The different varieties of congestive fever are not improbably dependent on the portion of the sympathetic system which is chiefly affected by the spasm or paralysis, as the cerebral or gastro-enteric. The conditions of spasm and paralysis do not always succeed each other with the regularity observed in an ordinary chill, since this pathological irritation, whatever its nature, may produce, according to its degree, like galvanic irritation in experiments, contraction or dilatation.

The paralysis of the trunks of mixed nerves, and that due to lesions of the cerebro-spinal centres, has been seen to produce in many cases an elevation of temperature which may last for a long time. The apparent exceptions are due partly to the observations being made at too late a period after the injury, and partly to the lesion giving rise to irritation, rather than paralysis of the vaso-motor fibres.

The experiment of Waller, already described, shows the effect of paralysis of a nerve-trunk from cold to be an elevation of temperature in the parts supplied thereby. I have repeated this

* Journal de l'Anatomie et de la Physiologie, 1866, p. 188.

experiment by placing the inner part of the elbow in a freezing mixture. As the paralysis of sensation and motion in the little and ring fingers became more complete, the temperature between them rose from below the graduation of the thermometer (probably about 80°) to 94°, when the not very comfortable experiment was stopped. The little and ring fingers were very much warmer to touch than the others of the same hand, and manifestly more vascular.

Of cases of paralysis of a single nerve may be mentioned the following:—

"A patient cut the inner side of the index-finger deeply; and after wound had healed, the distal part of finger was 2° hotter than the corresponding part of middle." *

Romberg † observed elevation of temperature of 1° R. (=2¼° F.) in the leg of a patient who had undergone a section of sciatic nerve and removal of a neuroma for neuralgia. This was three years after the operation. In a carefully observed case of anæsthesia of the fifth pair on the right side, described by Dr. Franz von Meyer, the temperature of the anæsthetic side of the face was throughout from 1°–1¹° R. (2¼°–4° Fahr.) higher than that of the opposite side.‡

As we approach the brain we meet with many more cases. Colin § records a case of spinal hemiplegia, with considerable increase of temperature on the paralyzed side. Death occurred suddenly, and there was found in the right anterior column of the cord a softened spot the size of a filbert, containing a small, soft, apparently recent blood-clot.

Copland ‖ speaks of increased temperature in paraplegia from organic disease of the cord as the rule, and Flint,¶ as an occasional condition.

Brodie *¹ records a case of forcible separation of fifth and sixth cervical vertebræ, with laceration of lower part of cervical spinal cord. The temperature between scrotum and thigh was 111° Fahr.

* Handfield Jones, Clin. Obs. on Funct. Nerv. Dis., p. 29.
† Nervous Diseases of Man; Syd. Soc. Trans., vol. i., p. 203.
‡ Ibid.
§ Year Book of the Syd. Soc., 1862.
‖ Med. Dict., vol. iii.
¶ Practice of Medicine, 1867, p. 632.
*¹ Works, vol. iii., p. 105.

In a case reported by Waters,* a man was struck on left side of head by capstan-bar. The next day there was numbness of right side of face, and right arm and leg. A partial loss of power was observed in these parts, and the right side was of a higher temperature than left. The autopsy disclosed a clot of blood lying on right side of posterior aspect of medulla oblongata, and two lacerations, of which the first divided the restiform body of the right side transversely, involving the whole or nearly the whole of its fibres, and a portion of gray matter on the floor of fourth ventricle; and a second, situated to the right of rib of calamus scriptorius, dividing posterior pyramid and tract outside of it.

Weber's † case shows a lesion of the portion of brain supposed by Budge to contain the vaso-motor centre.

There was paralysis of right side. May 8th, left pupil wider than right, both contract imperfectly under influence of light, left much more so. May 31st, patient felt right arm and leg warmer than left. As measured, temperature in axilla and bend of elbow was about 5° Cent. higher.

Left crus cerebri contained an oblong clot of blood in internal half.

Charcot ‡ observed redness and elevation of temperature with vascular dilatation in cases of flaccid hemiplegia. "A diminution of temperature is connected with spasm of small vessels, coinciding with contraction, in cases where an irritative lesion accompanies rupture of brain. When the hemiplegia is accompanied from the beginning with contraction, the temperature at first falls, in two or three hours rises, and becomes equal, then superior, to healthy members."

A case is reported by Ogle § of motor and sensitive paralysis on right side; complete loss of motor power in right arm, hand, and fingers. Diminished sensibility on whole right side. The temperature of right arm was considerably higher than on the opposite side. Ever since he had been ill there had been increased

* Med. Chir. Trans. xlvi., p. 115.
† Med. Chi. Trans. xlvi.
‡ Gazette Hebdomadaire, 11 Juillet, 1867.
§ Trans. Path. Soc., vol. ix.

heat of that side of the body. All the fingers of the paralyzed side were much swollen.

Folet* has collected a considerable number of observations on temperature in paralysis, chiefly in hemiplegia.

In one there was paralysis of right arm, which was from five to eleven degrees warmer than left. There was osteitis of second and third cervical vertebræ.

Several cases of recent hemiplegia gave an excess of heat on paralyzed side; and another series, observed at various times after occurrence of paralysis, one so long as three months, confirm the first-mentioned, except in two instances, where the temperature was equal.

The following table of cases† observed at the Bicêtre shows the condition in old cases. The second column gives the duration of disease, and the fourth the difference of temperature, that of the healthy side being subtracted:—

1	3 years	Little or no atrophy	+ 0.8
2	1 "	No atrophy	+ 0.6
3	20 months	Slight deltoid atrophy	=
4	4 years	Atrophy slightly advanced	=
5	6 "	" " "	=
6	Old	" " "	— 0.2
7	8 years	Atrophy	— 0.2
8	7 months	Slight atrophy	— 0.6
9	18 months	Atrophy	— 0.6
10	7 years	"	— 0.8

In a large number of cases, so large as sometimes to be held to constitute the rule, to which the cases I have quoted are an exception, the temperature is found to be lowered.

It is probable that this is owing to the examinations being made at too late a period to detect a primary rise. It has been supposed that the temperature in paralysis always goes through two stages, in the first of which it is higher and in the second lower than normal. Eulenburg and Landois,‡ who develop this theory at some length, have observed these stages after

* Gazette Hebd., 1867, Nos. 12 and 14.

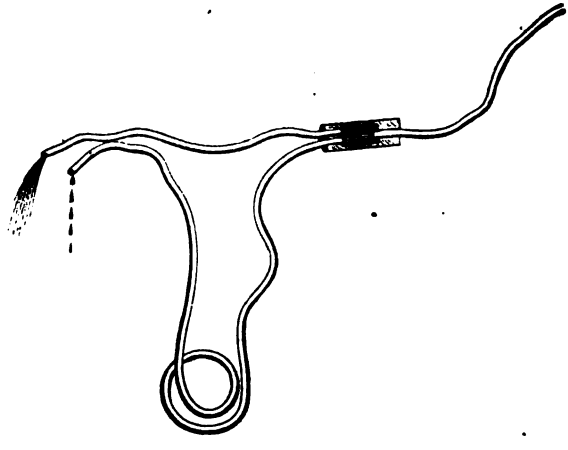
† Folet, loc. cit.

‡ Wiener Med. Wochenschrift, 1867, No. 65.

section of the sciatic in animals. The cases I have quoted, however, show that the second stage is not always reached even after some months, or even years.

When the second stage of diminished temperature exists, it is due to several causes. First, from the paralysis of muscular nerves, the warmth produced from muscular action is lost, as well as that which arises from their ordinary nutrition. Second, from the want of intermitting muscular pressure on the veins, the circulation is slower. It is principally to these causes that the slight dimunution of temperature shown in the table just given is due. This table shows the temperature to be lower in the cases in which atrophy had taken place.

The most important cause, however, in many cases is to be found in the consequences of the vaso-motor paralysis itself, and the relation of the vascular tone in the paralyzed parts to the general arterial tension and to the heart's action.



There exists in some cases a passive hyperæmia, an increased amount of blood in the affected part, but not an increased circulation in all portions of it. This is not, as seems to be implied by Eulenburg and Landois (loc. cit.), from the want of the active propelling force of the smaller arteries, since this force does not exist, but from the want of the proper degree of constant contraction or tone.

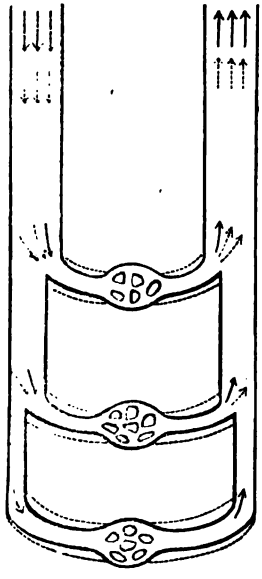
The manner in which want of tone produces, in a vascular

territory much smaller than that affected in the "Klopfversuch" of Goltz, a partial stagnation or passive congestion of the blood, may be illustrated by a simple experiment.

If a stream of water be conducted to a tube with two openings, one of which connects with a long tube and the other with a short one, so long as all is open the preference will be for the short tube, and under any pressure the larger quantity will flow from this. By regulating the pressure the water may be made to run in a stream from the short one, while it only drops slowly from the other.

A pressure on both tubes will now equalize the current, not only diminishing the amount passing by the short tube, but increasing that issuing from the long. The pressure may be considered the tone, and the widely open condition of the tubes corresponds to vaso-motor paralysis.

In the actual vaso-motor paralysis, as well as in this counterfeit thereof, the larger arteries changing but little in capacity, we have a much greater area through which nearly the same amount of blood may pass and return to the heart. Consequently the blood chooses, as in the experiment, the shorter path, leaving the extremities with their longer arterial and venous circuits subject to congestion.



The accompanying diagram may serve to illustrate the point further, the black lines representing the normal condition, and the dotted that which obtains in cases of vaso-motor paralysis of considerable areas, especially in paraplegia.

The experiment just described may be made to illustrate the effect of a varying propulsive force.

If a strong current pass through the single tube, a brisk stream will flow through both the others, though with a difference in favor of the shorter. This may represent a vigorous heart with vaso-motor paralysis.

If the tubes be both compressed, the circulation will be

equalized, or nearly so; the current in the longer tube becoming more rapid. Vigorous heart, normal vessels.

If the stopcock be so adjusted as to send only a small stream, the short tube will, as already described, discharge much more rapidly than the longer one, which, although full, runs little or none. This may represent a weak heart with want of vascular tone, as in old paraplegia.

If the tubes be partly compressed, both run nearly alike; and we have, as is seen in many cases, the circulation equable, but not vigorous. A fifth case, not yet mentioned, may consist in a degree of compression so great as to check the circulation through both tubes, not merely equalizing it, representing vascular spasm.

Cases of paraplegia from injury to spinal cord may be arranged to agree with these varying conditions.

It should always be remembered that motor paralysis does not in all cases prove vaso-motor paralysis.

Probably in all cases, certainly in many, the temperature of paraplegic limbs is at first raised, sometimes very much. This is not wholly due to vaso-motor paralysis in the parts, as is seen by the fact that the temperature sometimes surpasses the normal maximum of the body, but to fever, or causes not so directly traceable to the injury. This temperature may remain raised for some time, may fall, or may be, as is probable from the parts concerned being otherwise perfectly normal, within the range of health.

When it is raised, or when it is normal, the integrity of the circulation is preserved by the good general health of the patient, the heart being able to carry on the whole circulation, and the nutrition of the tissues having suffered but little, calling for a full supply of blood.

When it falls it is the effect of a generally weakened circulation, being unable to force the blood through the remoter ramifications of a dilated vascular system. The influence of temperature on the circulation, as well as that of the circulation on temperature, is not to be overlooked.

The sensibility of paraplegic limbs to the influence of external temperature, and the marked and rapid changes undergone by them, as described by Mr. Hutchinson,* may be due to the more

* London Hosp. Rep., vol. iii., p. 355.

ready yielding of distant vessels, to the local stimulus of exposure to cold air, and the diminution of tissue metamorphosis under a low temperature; the circulation in these cases being more active in the vessels nearer the heart, which being dilated allow the blood to complete its circuit without passing through remoter regions.

The very high temperature to be observed soon after injury to spinal cord, is exemplified in the case quoted from Sir B. Brodie,* of forcible separation of fifth and sixth cervical vertebræ, and laceration of lower part of cervical spinal cord. The patient lived twenty-two hours. The temperature between scrotum and thigh was 111° Fahr.

Mr. Hutchinson † also mentions the temperature in a case of fracture at fourth dorsal vertebra. On the day after the injury, ankles were 101°, palms 104°. Two days after, the face was dusky; hands, feet and legs, although hot to touch, were quite pale.

It is quite evident in this case, from the excess of heat in the hands over the feet, the duskiness of face, and paleness of limbs, that the state of things was more complicated than a simple vaso-motor paralysis of parts below the injured spot. The location of the injury seems, however, to favor the application to human pathology of Schiff's experimental data as to the vasomotor supply of hand.

Other cases referred to by Mr. Hutchinson show that increased temperature may be observed for four or six weeks after injury, even after muscular atrophy has begun to take place. The temperature in these cases was, however, not constant, being preceded or followed by great changes.

Two cases are recorded in which, from no mention being made of coldness, and from the normal nutrition of the parts, it is fair to infer that the normal temperature was preserved. Accuracy of observation, equal to that attained in hemiplegia, is not possible in paraplegia, since there is no fixed standard of comparison, and many circumstances beside the paralysis affect the temperature of parts distant from the centre of circulation and heat.

* Works, vol. iii., p. 105.

† London Hosp. Rep., vol. iii., p. 344.

Flint* quotes a case reported by Professor Childs, of a man who became completely paraplegic as regards motion and sensation, from a fracture with displacement of twelfth dorsal vertebra. The patient lived fifty-three years after. The lower limbs were moderately atrophied, but the condition of the nutrition of the limbs is shown by the fact that a fracture of both bones of right leg united in the usual time, and without trouble.

About a year after the injury the anterior crural nerve recovered motor and sensitive functions. No mention is made of the circulation. Is it not fair to suppose, either that the vaso-motor nerves of the limb acted by anastomosis from the anterior crural, or what is more probable, that a considerable portion of the vaso-motor nerves arising above the seat of injury escaped rupture or pressure, corresponding again with Schiff's experiments?

Neither of these explanations is, however, possible in the case quoted by Erichsen† of paraplegia from a blow, principally falling on lower cervical spine and shoulders. "A complete paralysis of sensation and motion, of all parts below this, was the result." "There was a sensible increase in the frequency and volume of the circulation." "The lower limbs retained their natural heat and physical development."

The legs of this man were both amputated near the hip, and the wounds healed by first intention.

There seems in this case still to have been some sort of control over the vessels, sufficient to maintain nearly their normal tone, for a "sensible increase of circulation in volume and frequency" does not correspond to a complete vaso-motor paralysis; but in what way this could be exerted is difficult to see, since the paralysis of sensation and motion was much greater than in many cases where the circulation is much more affected. It was precisely the case for great disturbance, both of the circulation and of nutrition; of the circulation, because the abdominal tract of vessels, containing a large proportion of the whole mass of blood, must have been involved; and of nutrition, because motion and sensation were so completely destroyed. It must in some points be considered for the present inexplicable, though a

* Practice of Medicine, p. 637.

† Railway and Other Injuries of the Nervous System, p. 33.

thorough examination of a similar case, after the course of the vaso-motor fibres shall have been accurately determined, will probably clear up the mystery.

The secondary fall of temperature in paraplegia is well known, being by some considered the rule, by others a not rare exception.

In a child with fracture of spinal marrow, in dorsal region, the temperature of sole was 29.5°C., popliteal space, 35.5°C., arms, 38.3°C., axilla, 37.90°C.*

Romberg † describes, after Heine, cases of paraplegia, probably the effects of meningitis in children, where the temperature of legs and feet sinks as low as 14°–17°R. (67°–70° Fahr.), and the color of the surface is livid.

Another and very remarkable group of cases, in which the temperature is materially lowered after injury to nerves, is that constituted by injuries mostly to the fore-arm, and mostly by glass. Mr. Hutchinson ‡ records seven cases, in all but the first of which, a nerve of the fore-arm was cut by a piece of glass. In the exceptional case the arm was sawn by a circular saw. Mr. Lyle § records a case of the same kind, and Mr. Adams ‖ a case in which a tumor, found after injury by glass to median nerve, was removed, the coldness persisting. Of two cases recorded by Earle,¶ one was an excision of a part of ulnar nerve for neuralgia; the other, paralysis of brachial plexus from fractured clavicle. Eulenburg and Landois suppose that, in cases of this kind, if the observation had been made soon after the injury a rise of temperature would have been observed, and that the fall in the second stage is the usual and necessary consequence of vaso-motor paralysis.

There are a number of circumstances which oppose this view, and go to show that these cases constitute a class by themselves, and are not to be explained by simple vaso-motor paralysis. We have seen, by cases already quoted, that the temperature sometimes remains after a considerable interval higher than normal from sec-

* Eulenburg and Landois, Wien. Med. Woch., 1867, No. 65.

† Nervous Diseases of Man, vol. ii., p. 361.

‡ Lond. Hosp. Rep., vol. iii.

§ Med. Times and Gazette, March 21, 1863.

‖ Dub. Quart. Jour. of Med. Science, May, 1848.

¶ Quoted by Eulenburg and Landois, Wien. Med. Woch., 1867, No. 65.

tion of a nerve; in Romberg's case three years; in that quoted from Handfield Jones, after wound was quite healed; in another,* given by the same author, of a wound of the leg by hay knife, "ever after." It is singular that in none of the nine cases mentioned, supposing them to be similar in character to other paralyses, should the first stage have lasted long enough to be observed.

In several of these cases, the loss of temperature extended to parts above the position of the injury. In some, it is mentioned that the wounds were long in healing, and in others that the cicatrix was adherent to parts beneath.

Mr. Hutchinson describes the condition of the affected members as follows: "There is always a remarkable loss of heat in a part which has lost sensation. Patients state that in cold weather anæsthetic fingers become very cold and blue, and in many cases he was able to demonstrate this fact. In warm weather heat never comes up to standard of unparalyzed parts. Even inflammation does not bring heat up to normal standard. While a paralyzed part can be cooled to almost any amount, it cannot be raised by artificial heat beyond a certain point, a point much below the maximum of its uninjured counterpart."

These are not, as has been several times remarked, and as several of Mr. Hutchinson's own cases of paraplegia show, symptoms of simple paralysis, neither are they the symptoms of contraction of the vessels of the part, which is plainly, as to quantity, well supplied with blood. They much resemble, however, the condition of a rabbit's ear, in which the blood, though abundant, has ceased to circulate, from a ligature at the base of the member.† In such an ear the temperature may have been raised by section of the sympathetic, and it would resist the action of cold longer than the other; but if the circulation is stopped, it loses its excess of heat, and falls below the standard even of the unoperated side.

It seems possible that we may find, in an abnormal or irregular contraction of the blood-vessels, either high up in the arm, from reflex action, taking its origin in the irritation of the cicatrix, or lower down, from reflex or direct irritation, a partial analogue to the ligature.

* Op. cit., p. 29. † See Section III. of Physiology.

This hypothesis, or something similar to it, finds support also in cases to be mentioned in another section, where nutritive disturbances follow undoubted irritation of nerves, and are terminated by section of the nerve.

An example of a limited vaso-motor paralysis, of reflex origin, is seen in the red spots on the cheeks in cases of pneumonia, or other thoracic inflammations. Gubler has shown that these spots are attended with increase of temperature, which, as well as the redness, is greater on the side of the disease.

The effects of irritation of vaso-motor nerves correspond with considerable exactness in pathology to those observed in experiments upon animals.

Waller * found that pressure on the nerves of the arm, produced either by hanging it over the back of a chair or allowing the nerve experimented on to rest on a hard projection, caused a diminution of temperature, not only at the time, but for some days after. The statement of Charcot, already quoted, that, in paralysis attended with irritative lesion, the temperature of paralyzed parts falls for two or three hours, and then rises, gives another example.

The principal excitants of vaso-motor spasm, or of a less violent and more enduring contraction, are of reflex origin.

Abundant examples—unfortunately, however, too indefinite to have much scientific value—might be found in the various symptoms of cold feet and hands, of chills and flushing, to which feeble patients, especially females, are subject. If it should ever be discovered through what special channels, if any, the morbid irritations reach the centres, and become vaso-motor stimulants, these phenomena may become more useful in diagnosis. For the present, they can only be regarded as evidence of irregular action, connected, like many other spasmodic phenomena, with impaired nervous nutrition.

A special form of vaso-motor spasm has been described by Nothnagel,† which causes gradually increasing coldness of hands, with anæsthesia and neuralgia. Motility is not impaired, though the finer movements are so from defective sensibility. The patients are women, and ascribe their malady to cold water. The

* Proceedings Royal Society, vol. 12, p. 89.

† In Year Book of New Syd. Soc., 1865.

spasm does not extend to radial arteries. Heat, friction, and galvanism cure it.

I may mention, in this connection, the case of a lady who has been in somewhat delicate health for some time, especially since a miscarriage about a year and a half ago. She has lately suffered principally from attacks of palpitation and fainting. She has often attacks of extreme coldness, limited to left side, both hand and foot. At the time of my examination, the left hand was perceptibly colder, and the left radial pulse smaller. She also suffers from pain in middle finger of left hand, the nail of which is somewhat misshapen, a deformity which she says came on within a month before the examination. A similar but less severe neuralgic affection of the corresponding finger on the right side, some time ago, was also accompanied by a slight deformity of nail.

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* [illegible] 12, p. 89.

† [illegible] 1863.

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SECTION IV.

HYPERKINESIS cordis,* or nervous palpitation of the heart, independent of organic lesion, may be properly regarded, in the light of the experiments detailed in the physiological portion, as a neurosis within the range of the sympathetic; and is undoubtedly an excitation of the nerve centres acting through the various cardiac branches, especially the lower or "accelerator." Perhaps the pneumogastric may also share in the phenomena, since it has been shown that slight irritation of this nerve, especially by the constant current, accelerates the heart's beats.

The dependence of this affection on psychical emotions, and also its occurrence as a symptom in other diseases, as for instance dyspepsia, seem to agree with the action of other portions of the sympathetic.

Although strictly beyond the range of this essay, it may be stated, for the sake of completeness, that the stoppage of the heart from over-stimulation of the pneumogastric is a pathological, as well as an experimental phenomenon.

It is very probable that many cases of death from shock of various kinds depend on a reflex stoppage of the heart through the pneumogastric. Several instances are given by Brown-Séquard.†

"That condition of the heart is to be called subparalysis, where, in consequence of an altered innervation, the cardiac activity is diminished, in distinction to the cases in which a malady of the heart itself is the cause of the diminished contractile power."

"The causes of subparalysis of the heart are either central, proceeding from the brain or spinal cord, as is often the case in diseases of these organs, specially concussion; in deep ether or chloroform narcotism; in poisoning by digitalis, veratrin, &c.—in so far, that is, as occasion is given in the central organs to a disturbance of innervation in the heart-nerves—or the causes

* Oppolzer's Vorlesungen über Specielle Pathologie und Therapie, I. Band., p. 259.

† Phys. et Path. of Nervous Centres, p. 159.

are peripheral, that is, situated in the course of the cardiac nerves." *

A striking example of the latter form, referred to by Oppolzer, is quoted more at length by Romberg † from Heine.

The patient complained that his heart often stood still. He was soon "able to demonstrate the fact, for there was an intermission of five or six beats of the heart and the pulse. He had a presentiment of the coming arrest in the shape of internal restlessness and oppression; that when the stoppage took place, a violent pain seized both sides of the thorax, extending to the neck, and then passing up to the nape and the head. He knew nothing, except mental emotions, which had a tendency to produce the attack; that, without any known cause, the malady had irregular periods, during which it occurred much more frequently than at others."

"The most accurate examination of the heart during the free intervals failed to demonstrate any abnormity in the rhythm, the sounds, or the extent of the cardiac dulness."

"The attacks gradually increased in frequency, endured longer, and the headache and pain at the cervix became persistent."

"The nervus cardiacus magnus, which ascends from the plexus, lying between the descending aorta and the pulmonary artery, was woven into a black knot, of the size of a hazel-nut, by the lax, pale grayish cord, forming the cardiac nervous plexus; before its entrance into the knot, the nerve was thickened. The branches of the left vagus, which descend upon the anterior side of the left bronchus to the pulmonary plexus, proved to be dragged in the same way by an underlying nodulated, dark blue lymphatic gland."

There were, besides these, lesions of brain and spinal cord, the cervical portion of the latter being particularly voluminous. The medulla oblongata was pushed over to right side.

The explanation of this case given above by Oppolzer, cannot, unfortunately, be accepted without acknowledging that the symptoms may have been caused by the pneumogastric, either from the tumor in its course, or the compression of its origin, the

* Oppolzer, op. cit., p. 272.

† Nervous Diseases, vol. ii., p. 340, from Müller's Archiv., 1841.

medulla oblongata. The latter cause, however, can be almost with certainty excluded by the entire absence of other symptoms referable to this region; but as a tumor may give rise, according to the degree of its pressure, to either irritation or paralysis, the presence of a growth on both sympathetic and pneumogastric detracts from the simplicity of the explanation.

Voluntary control over the heart's action can hardly be considered pathological, but is certainly abnormal. Fontana is said to have been able, by practice, to move the iris and auricular muscles (the latter is not very uncommon), and also to retard or accelerate the beat of his heart, without marked movement of the muscles of respiration.*

The case of Col. Townshend † seems to have been of this kind. This gentleman, for many years an invalid, could die, or expire, as he called it, when he pleased, and yet by an effort he could come to life again. A short time before his death he tried the experiment in the presence of three medical men, one of whom, Dr. Cheyne, relates the case. The pulse and respiration gradually ceased, until the observers supposed that he was dead, and were preparing to leave him, when they found his heart again beginning to beat perceptibly, and breathing to recommence. He recovered his usual condition, but died in good earnest on the afternoon of the same day.

* Romberg, Op. cit., vol. ii., p. 249.

† Dunglison's Human Physiology, 1832, vol. ii., p. 134.

SECTION V.

LESS is known of the pathological than of the physiological action of the nervous system on the secretions, imperfect as our knowledge of the latter is in some points. Abundant instances, however, to show the reality of such action, are to be found in pathology as well as in experiments, but for the demonstration of its method we must rely principally on the latter source. The secretions of the skin, mucous membranes, salivary, lachrymal, and mammary glands, liver and kidneys, are all more or less influenced by affections of the nervous system.

There are three sets of vascular and secretory phenomena to be observed in the skin, their difference probably depending on the fact that the skin is really an assemblage of several distinct organs.

The first of these, or the condition of physiological activity, consists in hyperæmia, heat of surface and free secretion. When affecting a considerable portion of the surface, it is a consequence of, and protection against, extreme heat, and may also, as is well known, be caused by mental emotions of various kinds.

Some singular cases of local redness and sweating have already been quoted, of which perhaps the most remarkable is that of the lady mentioned by Bartholow, where the temperature was found 5° higher on the affected side of face.

In some of these it has been possible to trace a connection with an organic lesion affecting the cervical sympathetic, as in the case quoted from Ogle. In this case, however, the flushing followed the sweating.

In others a connection with irregular or weakened nervous action is rendered very probable by other symptoms. Such are the case of Dr. Russell, already quoted, of flushing and sweating preceding an epileptic attack, and one mentioned by Handfield Jones,* where a hemiplegic patient found that after a lamp-

* Functional Nervous Disorders, p. 280.

bath the perspiration was much more profuse on the paralyzed side; also a case of Dr. Ramskill,* where unilateral perspiration with increased heat in an epileptic could be excited by heat or the constant current. In the last case the pupil, contrary to what is to be expected from paresis of sympathetic in neck, was dilated. Jones † speaks of a case where a patch of redness with profuse perspiration occasionally appeared on the fore-arm of a lady who suffered from frontal neuralgia and valarious fever.

The occurrence of heat without perspiration, the second of the three conditions spoken of, is in the most distinctly marked cases only a temporary phenomenon. In some fevers, however, it lasts for many days, and is sometimes succeeded by a distinctly marked sweat; at others passes gradually into the normal condition.

Absence of perspiration, however, is not a necessary concomitant of high febrile temperature, as is seen, for instance, in acute rheumatism.

It is to be remembered, in explaining the phenomena of fever, that the increased perspiration of the third stage assists in reducing the cutaneous temperature, and is not merely a consequence of such a reduction.

The same fact is to be borne in mind in regard to the cold sweats of various debilitating diseases, especially phthisis, or occurring after malarial fever.

These sweats, as well as those accompanied by heat and redness, may depend on mental emotion, and may also be local, as shown in another of Bartholow's cases.

In order to reconcile these various sets of phenomena with physiological conditions, it might be thought necessary to invoke the assistance of the direct secretory nerves, but the cutaneous secretion is perhaps the very one of the whole body which can be most easily accounted for on the simplest principles of exosmosis, since it is so largely composed of water and saline constituents, and the peculiar organic matters are in so minute a proportion.

It seems very probable that different sets of nerve-fibres con-

* Med. Times and Gazette, Ap. 7, 1866.

† Op. cit., p. 281.

trol the circulation in the papillæ near the surface which give heat and color, and in the coils of the perspiratory glands which produce the secretion. The derivative circulation of Sucquet may have an important share in determining the structure which shall receive the principal part of the blood.

The first condition, that of heat, redness, and secretion, is accounted for by paralysis, partial or complete, of both sets of fibres.

In the second condition, that of fever, heat and redness without secretion, after the first stage or that of irritation, in which the whole cutaneous vascular system is contracted as well as the arrectores pili, the superficial vessels, those supplying the papillæ, first relax; the heart, which is under the control of the nervous system independently of the vessels, and which is further stimulated by the internal heat which even during the cold stage is steadily rising, forces the blood through them and through the arterio-venous channels of Sucquet, making the skin red, and carrying to it the abnormal heat of the interior of the body, the vessels of the perspiratory apparatus in the mean time remaining contracted.

At length these also yield, and the blood circulating freely, its watery and saline constituents filter through the gland cells, and, evaporating from the surface, assist in reducing the temperature.

In the third condition, secretion without heat and redness, vasomotor paresis takes place from general nervous depression. The heart does not act more powerfully than it should, but the contrary. The glands are supplied with a sufficient quantity of blood to carry on the secretory process, but there is no such rapid circulation as in fever or in physiological sweating. The evaporation of the sweat tends to cool the surface and diminish still further the activity of the superficial circulation.

A very good instance of nervous influence on the secretion of mucous membranes can be furnished from the writer's personal experience. He is subject during summer and autumn to the "summer catarrh," or an extreme irritability of mucous membrane of posterior nares and palate.

When these parts are in a condition of comparative comfort, with little secretion, a slight exposure to cold of various portions of body, as shoulders or even arms thrust from under the bed-

clothes in the morning, or a cold draught on the back of the head and neck, determines an immediate congestion, as shown by the stoppage of nasal air-passages and a copious secretion of mucus with sneezing. The action in this case must be a primary dilatation of vessels, probably arising from irritation transmitted to nervous centres, there to become a vaso-motor paralyzant traversing the fifth pair.

It is useless to multiply instances of abnormal secretion under reflex influence, as the fact that such secretions take place is undoubted; while its explanation would be only a repetition of what has already been said, and would add nothing to our positive knowledge or the clearness of our hypothesis. Many such instances are given by Brown-Séquard.*

Some conditions in human pathology have been found which correspond with more or less exactness to the experiments on the artificial production of diabetes.

The coincidence of nervous lesions with diabetes is by no means a rare occurrence; but in very many cases such lesions are undoubtedly secondary. It is at present not at all probable that any organic disease of the nervous centres can be shown to be the cause of all, or even a large proportion of cases in which sugar is constantly excreted. The steady progress of most diabetic cases, their resistance to all treatment but the dietetic, and the correspondence between the chemical constituents of the food eaten and the sugar excreted, are much more like the symptoms of a blood disease or derangement in the organs of assimilation than like those of a neurosis, and in the majority of cases anatomical evidence of nervous lesion would be wanting, while in those in which it would be found a part would be secondary.

Yet the connection of some cases of saccharine urine with nervous disorders is very clear.

Dr. Goolden † has admitted every case of epilepsy, paralysis, and chorea that has applied at his Hospital (St. Thomas', London), during his week of admission, and not only has he found sugar in a large number of cases, but he has found the sugar to disappear as the symptoms were relieved by treatment. He

* Phys. and Path. of Nerv. Centres, p. 152 et seq.

† Lancet, June 24, 1854.

found many cases of saccharine urine to be relieved by purging and counter-irritation to head.

Dr. Ogle* refers to a large number of cases of diabetes in which there was reason to suppose that some lesion of the nervous centres was concerned in the production of sugar.

In one of these Dr. Richardson found the symptoms much increased whenever the patient, who was a house-painter, had to look constantly upwards with the head thrown backwards, as in painting a ceiling. Kunkler relates a case where there were several tender spots at upper part of dorsal region of spine which disappeared on cupping. The sugar in the urine ceased to appear on the application of a blister to the neck, in spite of unchanged diet.

The most striking case, however, which corresponds very exactly to the experiments of puncture of medulla oblongata, was observed by Ogle himself.

A man aged forty-four entered the Hospital, June 20th, 1866. Two months previously he had an attack of erysipelas followed by general debility, and for some weeks before admission had noticed that his urine was in large quantity and that he had lost flesh. He passed 168 oz. of sp. gr. 1033 per diem.

He improved on mineral acids and cod-liver oil, the urine diminishing. Soon after he had twitching of right arm and right side of face. These convulsive attacks continued, the urine the mean time diminishing to 46 oz. Afterward it was passed in bed with the fæces. Shortly before death he had a slight erysipelatous condition of right side of face.

The medulla oblongata was examined by Lockhart Clarke. "To the naked eye the whole of the medulla oblongata appeared perfectly healthy, but on examining the sections under the microscope it was evident that the fourth ventricle, from the point of the calamus scriptorius upwards for about quarter of an inch, was the seat of a finely granular deposit, extending for some distance down the raphe, in which, as well as in the substance of the medulla oblongata on each side of it, numerous corpora amylacea were interspersed. In no other part of the medulla, nor in the third ventricle, could I perceive any abnormal appearance. The

* St. George's Hosp. Reports, vol. i., p. 157.

central part of the pons seemed more vascular than usual, but this might be accidental."

A case of diabetes is also described by Luys in which, after death, lesion of anterior wall of fourth ventricle was found.

Of the drugs which may produce a temporary diabetes, chloroform is perhaps the most important. Pavy found in all but one of twenty cases sugar in the urine after the administration of chloroform, though in only four was it present previously. Perhaps observation might discover cases in which the use of mercury is followed by the appearance of sugar in the urine, as found experimentally by Saikowsky.

Cases have been seen where intermittent fever was accompanied with diabetes occurring on the fever days, the urine at other times being free from sugar.*

* Eulenburg and Landois, Wien. Med. Wochenschrift, 1867, No. 97.

SECTION VI.

THE pathology of nutritive disturbance from nervous lesion must take a wider range than a mere examination of troubles known to follow section or irritation of sympathetic trunks, since but few facts of the latter kind are known, and yet, from the condition of the circulation, and the probable dependence of some lesions upon it, it is fair to suppose that a large portion of the morbid action following injury to mixed nerves depends on the vaso-motor fibres they contain.

It cannot well be doubted, however, that other influences are also at work in such cases,—whether, as some suppose, through directly "trophic" nerves; or through some not yet understood action of the other classes of fibres going to make up a mixed nerve, especially the sensitive; or, as is known to be sometimes true, merely accidental.

The observed facts are farther from putting this question on a firm basis than might be supposed from their number. For this there are several reasons.

These are, principally, the still remaining uncertainty of experimental facts with which to compare pathological data; the want of full records of the symptoms in each case from the beginning, including the state of circulation, temperature, sensation, and motion, and the nature of the wound or lesion, and of the cicatrix.

I shall attempt, even at a considerable risk of error, to distinguish between the effects of paralysis and those of irritation, thinking that such an attempt, though faulty, will be more useful, and do more toward clearing up the existing confusion, than a catalogue of the abnormal conditions of nutrition, which have been seen to follow injuries to nerves.

The importance of such a distribution is very great, and although the reason cannot always be shown why one nervous lesion and not another should be followed by impaired or perverted nutrition, yet the fact is sufficiently clear, that in many

cases paralysis of mixed nerves, and sometimes that of the sympathetic, is followed by no considerable nutritive disturbance.

To recur to our classical experiment, it is well known that section of cervical sympathetic is often followed by no perceptible change of nutrition in the ear. Bernard says that in feeble or sickly animals, this operation is followed by inflammation of mucous membranes on the affected side of head.

It might be supposed from analogy that vaso-motor paralysis, by increasing the supply of blood, would give rise to increased nutritive, as well as increased secretory action, that is, to hypertrophy. This increased nutritive action has been supposed the cause of the elevation of temperature, but, as we have said, there is no other evidence of rapid metamorphosis in parts affected by vaso-motor paralysis, and the more active movement of the blood is, as has also been shown, amply sufficient to account for the rise of temperature.

Two tissues seem to be to a slight degree affected with hypertrophies in paralysis; one of them, however, following injuries to mixed nerves, not clearly showing its connection with a special class of fibres. The same is also liable to the doubt as to the nature of the lesion, whether simply paralyzing or irritative, which has before been suggested, and which is of so much consequence in all these inquiries.

The hypertrophy of connective tissue which follows many congestions, also follows those which arise from vaso-motor paralysis. It is seen principally in the orbit of the eye, and in the thyroid gland, forming important symptoms in exophthalmic goitre, or Basedow's disease, and will be more fully spoken of in connection with that affection.

The hypertrophy of epidermoid tissues is seen in the rough and scaly condition of the skin in many cases of paralysis. Dr. Ogle * has spoken of the union of a divided carpal nerve in horse. The hoof grew to greater size than usual, probably owing to division of nerve.

These changes are slight, and not always present. An ob-

* Med. Times and Gazette, 1866, vol. ii., p. 475.

servation of Lister * seems to show that the doubt just suggested may find its application in these cases, or at least that some condition beyond simple vaso-motor paralysis is concerned. He says that neither œdema nor exfoliation result from permanent full dilatation, produced by operations upon the cord or roots of spinal nerves (of frog), while, on the contrary, both took place in the case of division of sciatic, in which the arteries completely recovered their contractility within half an hour, and presented during the next twenty-four hours precisely similar appearances with those of the other foot.

The occurrence of more or less sloughing in paraplegia is well known. Charcot † has observed many cases of hemiplegia of sudden advent, and terminated by death, in which sloughs took place upon the nates, chiefly on the paralyzed side.

It is evident that pressure is not the sole cause of these sloughs, for they do not occur in persons lying in bed for a much longer period from other causes. In Charcot's cases, they often appeared on the third or fourth day after the occurrence of the paralysis, and in paraplegia have been observed even earlier.

On the other hand, if the loss of direct nervous influence were the cause, they should be coextensive with the paralysis.

The condition of the parts, and the manner of formation of the sloughs, as described by Charcot, show that, although they do not begin by simple stagnation or hypostatic congestion, yet a disturbance of the circulation is the first visible symptom.

The circulation in paralyzed, and especially paraplegic parts, as has been shown in the section treating of this subject, although perhaps active, is, from the greater number of sufficient channels through which the blood is at liberty to pass, more unequal than in parts preserving the normal vascular tone. This condition, causing, as was shown, congestion of distant parts, permits also, with greater ease, interruption of the circulation from compression. This is not inconsistent with a free

* Proc. Roy. Soc., 1858, p. 607.

† Archives de Physiologie, No. 2, p. 308.

circulation in other parts, or even an increased temperature of the remainder of a limb,* since the current of the blood is not interrupted by the state of the vascular walls, but merely wants the impulse which confinement within narrower limits would give. If a certain quantity of blood comes through the abdominal aorta, and is then at liberty to follow any one of several different channels, which together are much more than sufficient to carry this quantity on into the veins, a much slighter degree of pressure will cut off the circulation in a portion of the tract supplied with blood by these vessels, than if, from greater "tone" and diminished calibre, all the arteries were needed to transmit the same quantity.

It is also to be noticed that, although no action of the muscular coat of arteries can produce dilatation, yet an artery which is in a state of partial contraction is less easily compressed than one whose walls are paralyzed and flaccid, as it is known that a small tense pulse is less compressible than a large soft one, although in the latter case the vessel is larger. This difference, though partly, is probably not wholly, due to the tension of the contents. This difference is also to be seen between veins and arteries in healthy limbs. Although the united capacity of the veins of a limb is greater than that of the arteries, it requires a less degree of compression to check the venous circulation sufficiently to produce congestion than to produce an equal retardation of the arterial.

Besides cases already quoted to show the normal or nearly normal state of nutrition in paralyzed limbs, others may be adduced where the usual processes of repair went on while sloughs were forming in the immediate neighborhood.

Two, precisely to the point, are mentioned by Paget: †

"I remember a man with nearly complete paraplegia, and distorted feet, the consequence of injuries to the spine, in whom some tendons were subcutaneously divided, and appeared to be healing; but a bandage being applied rather tightly, sloughing ensued at the insteps, on which the chief pressure fell, and extended widely and deeply to the ankle joints. Both the dorsal

* See Romberg's case already quoted. Nerv. Dis. of Man, vol. i., p. 205. Sloughs of heel and ankle, though leg was of higher temperature than the other.

† Surgical Pathology, p. 44.

arteries were laid open when the sloughs separated, and both the ankle joints, and the case presented a most striking example of the defective self-maintenance of paralyzed parts. But granulations formed after the separation of the sloughs, and the healing process went on slowly but uninterruptedly, till all was covered in with a well-formed scar. In another case, a girl, with softening of the brain, had sloughs on nearly every part of the body that was subject to even slight pressure; for instance, on the back of her head resting on the pillow, on her elbows, and heels; and yet, while several of these sloughs were extending with fearful rapidity, an ulcer which had remained after the separation of a slough over the patella healed perfectly."

The foregoing considerations show that neither pressure nor loss of nervous influence can be regarded as alone the cause of sloughing in paralysis.

The vascular paralysis which accompanies that of the voluntary muscles, and sometimes that of sensation, produces in the circulation of the part affected, a condition of less resistance to pressure. In this condition a very slight amount of pressure may be sufficient to prevent the free access of blood, and the consequent nutritive changes.

It is to be remembered, also, that into some, and perhaps many, of these cases the element of irritation may enter and assist in the production of eschars, if the parts supplied by irritated fibres coincided with those to which pressure is applied. An irregular contraction of blood-vessels would not facilitate the movement of the blood, nor tend to relieve the condition of congestion, as these effects are obtained by a regular and uniform tone of the arteries.

As vascular paralysis produces increased secretion, and to a very limited extent hypertrophy, it might be inferred that irritation of the vaso-motor fibres would produce a corresponding depression of vitality. This is in fact the case; but, as in the opposite condition, the direct effect is limited.

Lesions of nutrition, as a consequence of irritation, are, however, if the explanations heretofore adopted in this essay are correct, much more varied than those following paralysis.

Several cases are known of unilateral atrophy of the face, or a portion of it; and these have been interpreted as dependent

on a nervous lesion, probably irritation of cervical sympathetic, causing contraction of blood-vessels.

Romberg * records a number of cases of this character. The left side of the face in a woman of 28 was much smaller than the right, and showed many signs of defective nutrition, such as thinness of hair, roughness and wrinkles of skin, pale, earthy color. Temperature, sensation, mobility, and secretion were however alike on both sides.

An acute exanthem, probably scarlatina, with affection of cervical glands, and swelling of left tonsil, had preceded this affection. The atrophy increased, and the patient became subject to religious mania.

In another case, in which some atrophy of left side of face succeeded an attack of hemiplegia of same side, the hair was white on the affected side up to the median line. The beginning of the attack was attended with some numbness of the left side of face, which afterward disappeared, with the exception of some remains in the left ear. Motion unimpaired in face and extremities.

A third instance, in which the atrophy was chiefly limited to parts supplied by the third branch of the fifth pair, showed irritation of the muscular and sensitive fibres of this pair, the former manifesting itself in irregular spasms of masticatory muscles, and the latter as hyperæsthesia, principally of first and second branches.

A fourth case consisted in atrophy, and falling out of hair on one side of head, following an injury, and in attacks of pain on same side.

These cases, although showing some probability of connection with nervous lesion, are not at all clear as to the nature of the affection. Two other similar cases, quoted by Eulenburg and Landois, as well as cases of congenital unilateral atrophy of body, of which a remarkable example is given by Broca, † furnish even less evidence of the intervention of the vaso-motor nerves in this singular phenomenon. It should be remarked that, in one of these cases, certain discolored spots exhibited a condition of venous injection.

* Cases given by Eulenburg and Landois. Wien. Med. Woch., 1867, No. 81.

† Journal de la Physiologie, ii., p. 70.

Brown-Séquard * has observed in animals a more rapid production of atrophy in cases where irritation of spinal cord has taken place, than where there is simple paralysis. "I have seen at least several hundreds of animals survive entire months after section of spinal cord, and present no other lesion of nutrition in the paralyzed parts than an atrophy, in general slow to show itself. In two cases, on the contrary, exostoses were formed at the point of section of the cord, and had compressed the inferior extremity. There was a considerable atrophy in five or six days, and a gangrenous ulceration of sacrum and some points of the thigh."

As has already been remarked several times, it is often uncertain how far irritation of vaso-motor fibres may be concerned in the sloughing of paraplegia.

In a former section it was attempted to be shown that the congestion and coldness of limbs after injuries to the nerves was not the result of a simple vaso-motor paralysis, but of irritation.

The various nutritive affections which have been observed to follow such injuries, are even more clearly to be explained in a similar manner.

The precise manner of their occurrence, as in many other instances, is not so easily to be made out, but it seems probable that the vaso-motor fibres are largely, if not solely, concerned. These cases do not speak strongly in favor of the direct "trophic" theory, since the lesions, though evidently connected with nervous influence, do not affect the whole of the parts supplied by a given nerve, but usually choose some portion of the affected member more exposed than the rest. The fact that the tips of the fingers are often the seat of an affection resembling whitlow does not show that pressure is the sole cause, but renders it probable that some local exposure combines with the nervous disturbance to produce inflammation.

Mr. Paget † mentions two cases showing the production of ulceration after irritation of nerves. In the first, after injury to the median and other nerves, with adhesion to adjacent tissues, and induration of both it and them, the patient had

* Journal de la Physiologie, ii., p. 114.

† Surgical Pathology, Am. Ed., 1860, p. 42.

constant pain, impairment of touch, contraction of fingers, and constantly repeated ulcerations at the back of the hand.

In the second case, "a man was at Guy's Hospital whose median nerve was compressed by excessive formation of bone after a fracture. Ulcerations on thumb, fore, and middle fingers could be cured only by so binding the wrist that, the parts on the palmar aspect being relaxed, the pressure on the nerve was removed. So long as this was done the ulcers became and remained well; but as soon as the man was allowed to use his hand, the pressure on the nerves was renewed, and the ulceration of the parts supplied by them returned."

These cases, though by no means unique, are clearer than many others, from the demonstration of the condition of the nerve. In the cases of section of the nerves of a limb, in which the temperature falls and the member becomes congested and livid, it is much more sensitive to many external influences, which cause more or less morbid action. Both cold and heat cause vesication much more readily than in healthy parts.*

It has been attempted to be shown by Samuel,† that continued irritation of nerves gives rise to inflammation of the parts supplied by it; this effect being supposed due to the "trophic" fibres contained in mixed trunks, and a proof of their existence.

The inconclusiveness of his experiments has, however, been shown by Tobias,‡ and by Weber.§ The former showed that the croton oil which Samuel applied to the threads which he tied around the nerves was in itself, even if not touching the nerve, capable of causing extensive inflammation in the surrounding tissues. He failed in other experiments in producing the effects which Samuel described.

Weber found that powerful electric currents radiated into surrounding tissues. He used, in order to produce irritation, thread, metal rings, or fine needles; and partly a continuous galvanic current, surrounding the nerve selected with a fine

* Romberg, Nervous Diseases, i., p. 202.

† Quoted by Tobias, in Virchow's Archiv, Bd. xxiv., p. 579.

‡ Ibid.

§ Med. Times and Gaz., 1869, Oct. 1, p. 370.

conducting wire. The results were, contraction of blood-vessels, decrease of temperature, lasting for a longer or shorter time, and occasionally venous hyperæmia, but never any symptoms of inflammation.

The decrease of temperature and venous hyperæmia may be noticed as being, of the experimental results described, the nearest to the injuries from wounds, especially of the fore-arm.

It is well known that after section of the fifth pair within the cranium, extensive changes of nutrition occur in the eye. The interpretation of this fact is, however, by no means simple. In such questions as this, where a physiological fact is to be reasoned upon, it makes a great difference whether the anatomy is perfectly clear, and yet in this instance physiology is depended upon to resolve anatomical questions; a state of things which much complicates the matter.

The special facts in doubt are the passage of vaso-motor fibres directly from the brain into the fifth pair, and the union of fibres of the same kind with it at the Gasserian ganglion, in the cavernous sinus or elsewhere.

Still further difficulties are introduced by the difficulty and uncertainty of the operation required when experiments are made on animals, and the scarcity of sufficiently observed facts in human pathology.

"Let us now examine these disorders of nutrition which section of the fifth pair causes."

"In the eye, where they are the most apparent, independently of the paralysis of feeling in all the other parts where the ophthalmic branch is distributed, it is seen that at the end of some hours the transparent cornea is no longer so smooth nor so brilliant. The prominence of the eyeball is greater, the cornea more convex; the enlarged vessels are marked out around the circumference of the conjunctiva, as in the figure."

"After the section of the nerve, independently of the causes mentioned above (place of section), and which, depending on the position of the lesion, may hasten or retard these accidents, we ought besides to mention that the alteration may be rendered more prompt or more slow by the general state of the animals experimented on; the alterations of nutrition appear sooner in enfeebled animals."

"Generally at the end of two days the cornea begins to become opaque. At the end of some days the opacity of the cornea becomes more and more pronounced; the engorgement of the conjunctival vessels increases; a swelling forms around the mucous membrane—a swelling which furnishes a purulent secretion; inflammatory symptoms show themselves—inflammation without pain or heat. The temperature of this side of the face is, on the contrary, less than that of the opposite side."

"These disorders become more and more pronounced; the spot which first showed itself on the cornea deepens, becomes a true ulcer; the cornea is perforated. Then the eye is emptied; the perforation of the cornea gives issue to the crystalline and the vitreous humor; it is a sort of melting of the eye. If the animal survives, there remains of the eye only a small, hard stump."

"There is frequently a mucous discharge from the nose, and the commissure of the lips on the side operated on." *

There are also ulcerations of tongue and lips.

Magendie states that the end of these disorders is gangrene of one-half the face, especially in cold-blooded animals; but Bernard has never seen rabbits live long enough for this, or even for the complete destruction of the eye.

The place where the section is made, whether before or after the nerve has passed through the Gasserian ganglion, is well known to considerably affect the result. It is much a matter of chance, however, where the section falls, as the space behind the ganglion is so limited.

The removal of the superior cervical ganglion retards the disorders of nutrition.

These changes have been supposed partly due to the paralysis of blood-vessels, like that which takes place after section of sympathetic, rendering the eye more sensitive, and, in fact, originating an inflammation. The condition of the eye, however, differs in several essential points from that obtaining in vaso-motor paralysis. First, the congestion increases. In vaso-motor paralysis it reaches its maximum in a few minutes. The

* Bernard, Syst. Nerveux, ii. p. 62.

eye is colder. The opposite would be the case if the blood-vessels were all fully dilated.

If the vessels were already fully dilated, removal of the superior cervical ganglion would have no effect. If the lesions were owing to an incomplete but uniform dilatation, removal of this ganglion would accelerate the progress of the ulceration by dilating them still more. On the contrary, it retards it. Although inflammation takes place in an ear whose nerves have been cut with greater rapidity than on the other side, yet it returns to the normal condition more rapidly also. The ulcerative process in an eye after section of fifth pair goes on in a continuously destructive process for weeks.

A long step towards the true interpretation, although not covering all the ground, was made by the experiments of Snellen * and Büttner.†

The former found no alteration in the cornea of rabbits after section of fifth pair, when eyes were kept closed and covered with the ears.

Büttner carefully protected the eyes by a watch-glass set in a little cylinder, and succeeded in entirely preventing inflammation. In one case intense inflammation came on, although the eye was completely protected. It was, however, different from that taking place when eyes were not protected, being more violent. The autopsy of this case showed that the ganglion was divided, but that there was another partial cut anterior to this. There was inflammatory redness of ganglion.

Magendie and Brown-Séquard,‡ after removal of medulla oblongata and Gasserian ganglion of frogs, found the eyes to remain perfectly normal for months, while the animals were kept in a very cool, damp atmosphere, and in the cellar.

A second explanation § refers this phenomenon to extraneous causes of a somewhat different character. The author supposes the irritation to be propagated from the wound by simple continuity of tissue, and to be due to the severity and uncertainty of the operation, it being possible that much injury beyond

* Review in Journal de la Physiologie de l'Homme, &c., vi., p. 208.
† Brit. and For. Med. Chi. Rev., vol. xxxi., p. 233.
‡ In Review of Snellen, loc. cit.
§ Cyclopædia of Anatomy and Physiology, vol. ii., p. 311.

that essential to the experiment is brought about. After describing the procedure, he remarks, "After such a proceeding, the question should rather be, what mischief has not been done, than what has." He found in certain experiments, which failed, for the removal of Meckel's ganglion, that in almost every instance the eye of the same side became bleared within two days; and in one case in which he succeeded, there was produced opacity of cornea and ulceration, which continued more than a month after the operation, though neither infra-orbital nor ophthalmic nerves had been divided.

While these facts show conclusively the large share which mechanical irritation has in the production of the peculiar phenomena—a share much greater than would be taken were it not for the anæsthesia of the parts—yet they do not cover the whole ground.

Some experiments (probably partly accidental) of Meissner,* seem to separate the portion of the nerve acting on the nutrition from the sensitive portions. In three cases where, after section of trigeminus in the cranium, the eyes of the rabbits were entirely without sensation, no ulceration or other disturbance took place, although they were entirely unprotected. The autopsy showed that a small thread forming the medial border of the nerve had not been cut.

In another rabbit, where sensibility was preserved, but cloudiness, injection, and inflammation of the eye had taken place, only a slight injury on the internal border of the nerve had been produced.

It is evident that a disposition to be easily affected by external injury is produced in the eye by section of the fifth pair in the cranium, but that this disposition is not due to simple vaso-motor paralysis.

The condition resembles in several points that existing in limbs whose nerves have been cut, and somewhat to that produced by irritation of nerve-trunks in the experiments of Weber (loc. cit.). There is in each the lowered temperature, and the venous congestion; and in the former cases great susceptibility to external injury.

* Quoted by Eulenburg and Landois in Wien. Med. Woch., 1867, No. 70.

It is, therefore, not going very far to assume that the essential feature is irregular contraction, with dilatation of blood-vessels, neither cutting off the supply, nor allowing free course to the blood current, but causing it to stagnate in dilated portions. This may take place in various ways, from the complicated connections of the fifth pair before it reaches the eye, and from its unknown ones in the cranium. These cases illustrate still further the fact of trophic disturbances being especially connected with lesions of sensitive nerve fibres; a fact which may be supposed to connect hypothetical "trophic" nerves with the sensitive, or which accords well with the abundant examples quoted in the physiological portion of this essay, in which irritation of a sensitive nerve causes either contraction, with subsequent dilatation, or primary dilatation of vessels through reflex action.

Many facts in human pathology, more or less complete, may be found to harmonize with the experimental data.

Paralysis of fifth pair does not always produce diminution of temperature,* nor ulceration of cornea. Cases of complete paralysis of fifth pair are reported by Mr. Dixon † and Dr. Horner, ‡ in which there was no ulceration of cornea. In the latter case there was blindness, from dropsy of brain; trigeminus was absolutely torn off, except a few filaments. Did those few filaments represent the slender thread left in Meissner's experiments?

Mr. Hutchinson § has twice operated on the eye in cases in which fifth nerve was totally paralyzed; and both wounds healed well.

In a case of Mr. Crampton's, reported by Sir C. Bell,‖ a lady had complete anæsthesia of one eye, so that she did not know when the cornea was touched, although a number of vessels on the surface of the eye immediately became injected with blood. She called it dead. No mention is made of any lesion of cornea, or any kind of nutritive disturbance.

* Case of Dr. Meyer, in Romberg, vol. i., p. 206, note (already quoted).
† Med. Chi. Trans., xxviii.
‡ Am. Jour. Med. Sciences, vol. iv., p. 58.
§ Lond. Hosp. Reports, vol. iii., p. 305.
‖ Philosophical Transactions, 1823.

[illegible] Section of [illegible] Ganglion of Fifth Pair.

[illegible] from [illegible] of complete [illegible] of [illegible] of [illegible] of [illegible] of [illegible] of [illegible] [illegible] [illegible]

and motion were completely lost on corresponding side of the face, on which side also attacks of erysipelas were frequent. There was opacity and ulceration of cornea, with disorganization of globe.

The autopsy showed enlargement of tuber annulare on left side, due to a tumor contained therein. The fifth and seventh nerves were compressed against the base of the skull.

Neither these cases nor experiments settle the question definitely as to the differences observed between the section at different points. Schiff * denies the existence of a distinction of this kind to the extent often supposed; Magendie † states that when the nerve is cut between ganglion and brain, changes are more slow to appear; and Bernard † has seen them entirely wanting when section is made in the brain itself.

It is not possible to say, in the cases observed in man, where the most essential lesion was situated; but in some of them, that which was most striking to the naked eye was behind the ganglion, and in others, where the ganglion was evidently involved, the ulceration was wanting.

It is clearly demonstrated, however, that anæsthesia is not the sole cause of the lesions, and that the condition is not one of simple vaso-motor paralysis, but allied to an irregular and partial dilatation of vessels causing congestion, rather than to a complete vaso-motor paralysis, which, when affecting only a small portion of the body, causes a rapid flow of blood, with increased vital activity, as much disposed to repair as to destruction.

It is hardly worth while to do more than mention various forms of inflammation, which, although probably caused by nervous action, are even more obscure in their pathology than those we have been discussing.

Such are the well-known ulcerations of the duodenum after extensive burns, and sympathetic ophthalmia, both from the [illegible]ions of the other eye and from the irritation of teeth, and [illegible]rious affections of children at dentition.

* Review in Journal de la Physiologie, vol. i. p. 206.
† Système Nerveux de Bernard, vol. ii. pp. 60 and 61.

Charcot * has collected some instances of affections of joints which seem to depend on disease of the brain or spinal cord. The synovial surfaces were congested, and in one case the median nerve of the affected side was enlarged and injected.

Dr. J. K. Mitchell† seems to have observed some facts of this kind under the guise of rheumatism. He says: "Local blood-letting is most potent when applied to that part of spine which supplies with nerves the parts in a state of active inflammation.

"Few failures will thus happen in thus treating acute rheumatism, and that success will diminish as, passing through chronic rheumatism, we enter on the ground of neuralgia, a disease which sometimes spontaneously disappears."

There are, beside the various lesions whose origin has been discussed, several diseases supposed to be connected with abnormal action of the sympathetic or vaso-motor nerves, which claim for themselves a somewhat more definite position in the nosology.

It was remarked that the principal instance of connective tissue hypertrophy, accompanying or succeeding vaso-motor paralysis, was to be found in Basedow's disease.

This singular affection is characterized when complete by three symptoms; rapid pulsation of the heart, protrusion of the eyeballs, and enlargement of the thyroid gland. The last two symptoms give it the name of exophthalmic goitre.

More or less anæmia exists generally, if not invariably. Other symptoms which have been noted are mental depression and imbecility, inability to sleep, amenorrhœa, abnormal increase of appetite, followed by anorexia, sometimes hysterical phenomena, and emaciation.‡

The theory which, at first on symptomatic grounds, attributed this association of phenomena to disorder of the sympathetic has been sustained by a much larger amount of anatomical evidence than can be urged in support of other similar theories, especially that of progressive locomotor ataxy already mentioned.

* Archives de Physiologie, Nos. 1, 2, 3.
† Amer. Jour. Med. Sciences, vol. viii.
‡ Flint's Practice, 2d ed. p. 330.

Before proceeding to any explanation of the pathology, some of the principal cases will be described.

The case reported by Trousseau * and also by Michel Peter † was one of the first.

A woman, aged 60, following grief from loss of friends and violent weeping, felt her eyes suddenly become prominent, and her thyroid pulsate. She also had epistaxis. Afterward she had intermittent fever, at which time the enlargement of thyroid disappeared. At time of observation (July 3, 1863) she had exophthalmos, and eyes were sensitive to light. The heart beat energetically, but less than at beginning of malady. Thyroid body a little voluminous. No trace of goitre. Some time after beginning of exophthalmos, patient experienced remarkable hunger, also had diarrhœa.

She died of apoplexy, of which the appropriate lesion was found in brain after death.

There was hypertrophy of heart and atheroma, commencing cirrhosis. The thyroid body was hard, of almost shining appearance, composed of fibrous tissue which "cries under the knife." Eyes were thrust out by cellulo-adipose tissue, a little more red than ordinary. The ophthalmic artery was no more flexuous or larger than ordinarily. Walls of cranium thickened and vascular.

Cervical sympathetic ganglia: superior and middle normal; inferior, especially on right side, larger and redder than usual; numerous vessels on its surface.

Numerous vessels in the interior in an abundant packing of connective tissue, in the midst of which are seen nuclei and fusiform cells. Numerous globules of fat; ganglionic cells rare, small, mulberry-formed, some reduced to simple granulations. Nerve fibres few. A section shows the nerve fibres as it were strangled.

Cardiac plexus with no apparent alterations, unless a little redness of constituent branches.

A second case is reported by Reith.‡

Andrew, G., *æt.* 24.

* Clinical Medicine, translated by Bazire, p. 579.

† Gaz. Hebdomadaire, 1864, No. 12.

‡ Med. Times and Gazette, Nov. 11, 1865.

Left eye prominent for some time; neighbors say as long as they can remember.

On the day after admission, right eye nearly as prominent as left. Effusion under conjunctiva. Eyeballs motionless; vision not lost. Pupils dilated. Has power over eyelids, but cannot close them. No paralysis. Œdema of neck and ankles.

Pulse 100. Thyroid body slightly enlarged.

Autopsy. Eyes very much protruded. Effused blood under upper (right) eyelid; orbital veins much congested. Half coagulated grumous blood at fore part of orbit above eyeball. Left orbit healthy.

Middle and lower ganglia on left side enlarged and hard. Sympathetic nerves on both sides, especially left, enlarged. Inferior thyroid and vertebral branches more enlarged than others.

Under microscope, ganglia seemed loaded with granular matter. Dorsal, lumbar, and semilunar ganglia nearly natural size.

A case very carefully observed, both before and after death, is reported by Dr. F. I. Knight.*

The principal points were the long duration of the case (twenty years), palpitation of the heart, and enlargement of left lobe of thyroid, which continued until death. At the first observation the principal troubles were palpitation and dizziness, and a high temperature (100° in axilla). No exophthalmos; no apparent anæmia. Examinations with sphygmograph and ophthalmoscope gave no results of special interest.

About a month after this, having experienced some improvement during use of iodine and iodide of iron, he was discharged from the hospital, and in a few days died suddenly.

The autopsy showed enlargement of thyroid gland, being mostly a unilocular cystic condition of the left lobe; the contents thin, yellowish, serous fluid.

The wall of cavity was thinly and unevenly coated with ragged fibrin, with here and there a bit of hard, calcareous matter. All the rest was normal, rather deep, flesh-red gland-tissue; showing nothing peculiar under the microscope; but

* Boston Medical and Surgical Journal, April 16, 1868.

in amount showing decided but not extreme hypertrophy. Marked general dilatation of heart, with no valvular or muscular disease. Spleen enlarged. Lungs showed no trace of tubercular or other disease (as was at first thought possible).

On a very careful microscopic examination of the cervical sympathetic ganglia, by Dr. S. G. Webber, the left lower was found broader than the right. "The nerve fibres were diminished in number and size, in proportion to the amount of connective tissue in the left lower ganglion as compared with the right." "The left lower ganglion also showed a deficiency of nerve cells." "They were also much smaller than in any of the other ganglia." "They were not so strongly pigmented as on the right side; hence, many may have escaped notice." "In most of the sections, from all the ganglia, were masses of pigment apparently without cell-walls." "No fat globules were seen anywhere." The nerve fibres were smaller on left side than on right.

The difference in the size of the cells in the ganglia is shown by the following table, each number being the average of ten measurements.

	Left.	Right.	Difference.
Upper.....	·00116 × 00084	·00122 × 00097	·00006 × 00013
Middle.....	·00103 × 00080	·00124 × 00091	·00021 × 00011
Lower.....	·00086 × 00062	·00140 × 00101	·00054 × 00039

"In several sections of right lower ganglion, there was a much larger proportion of fusiform cells than in any of the other ganglia."

In an unpublished case, reported to Eulenburg and Landois* by Biermer, he found far advanced atrophy of the right sympathetic, as the most important alteration.

In the case upon which Geigel † chiefly grounds his theory, which will be mentioned below, there were found a partially ossified gelatinous growth, arising from the spheno-occipital synchondrosis, and penetrating the dura-mater; a growing up of the central canal of spinal cord; and considerable injection of the finer and finest vessels. In the immediate neighborhood

* Wien. Med. Wochenschrift, No. 9, 1, 1867.
† Ibid.

of the central canal, especially on the posterior border, the substance of the marrow was rather firm, and there seemed a slight increase of the neuroglia present.

Both cervical sympathetics were surrounded by a thick, fatty sheath of connective tissue, but the microscope determined neither in nerves nor ganglia any morbid change, beyond an intense brown pigmentation of the latter, nor any increase of interstitial connective tissue.

There is evidently an abundance of facts to connect this disease distinctly with lesions, either of the cervical sympathetic nerve, or of the corresponding spinal centres, there being even in the case given by Geigel, and supposed by him to favor the last-mentioned hypothesis, some abnormal conditions of cervical sympathetic. When the minuteness, and yet perfect distinctness of the lesions found in some cases, especially Knight's, are considered, any examination of less completeness can have but little value as negative evidence.

Unfortunately, however, the symptoms do not agree among themselves, the over-action of the heart and the dilatation of the pupil being apparently results of irritation to sympathetics, while the exophthalmos and thyroid enlargement must be regarded as consequences of vaso-motor paralysis.

The exophthalmos might seem at first sight a symptom to be placed on the other side, but it cannot be regarded as the analogue of the exophthalmos of animals, observed on galvanization of the sympathetic. This is shown anatomically by the much less development in man of the fibro-muscular sheath of Müller, and physiologically by the fact that no such exophthalmos was produced by Wagner on galvanizing the cervical sympathetic in the neck of a decapitated criminal, although dilatation of pupil and raising of upper lid were observed. Neither is it analogous to the exophthalmos produced in frogs by section of bulb or optic tubercles.* This is the result of hyperæmia, to be sure, but of an hyperæmia giving rise to increased sensitiveness and stimulation of orbital muscles, which, in the frog's socket, act to protrude the eyeball.

The facts of the very sudden development of this symptom,

* Liegeois, Journal de l'Anatomie et de la Physiologie, 1864, p. 62.

and its occasional complete disappearance after death, together with the not infrequently observed condition of hypertrophy of the fatty cushion behind the eye, with sometimes increased density and redness, make it probable that the primary condition is that of simple congestion, afterward giving rise to increased fibrous growth.

The thyroid enlargement is undoubtedly due to the same state of things.

The increased action of the heart, however, cannot be explained in the same way. The portion of the peripheral circulation known to be affected is too small to have so marked an effect upon the heart, even if it acted in that direction. In many, if not most cases, the overaction of heart is observed before other symptoms make their appearance.

It is difficult to attribute this phenomenon to anything else than direct stimulation by the cervical ganglia, especially the lower, through the motor or "accelerator" nerves.

The difficulty of supposing irritation and paresis both to take place from the same lesion has been felt by Geigel, who removed it one step, without clearing it up, by supposing the primary lesion to be situated in the spinal cord; for which theory he thinks the case above quoted is an argument.

Aran * arrives at a theory of this disease as a sympathetic neurosis on somewhat different grounds from the more usually accepted.

He says that exophthalmic goitre is not essentially constituted either by an exophthalmos or swelling of thyroid body, but by a state of irritability of heart and arteries of neck, to which is soon added a dilatation and hypertrophy of the same organs. Preceding these are various troubles in digestive organs, secretory and nervous systems, which leave no doubt that there is some bond which unites them. This seems to be the great sympathetic.

It is well known that Trousseau considered this disease a sympathetic neurosis—probably a sub-paralysis. Oppolzer † considers this view as probably the correct one.

After considering all that has gone before, especially the

* Gazette Hebdomadaire, Dec. 4, 1860.

† Vorlesungen über spec. Path. und Ther., 1867, p. 262.

great importance of the cardiac symptoms, which cannot be due to sympathetic paralysis, our conclusion may be formulated as follows:

Exophthalmic goitre, accompanied by palpitation, which is increased in paroxysms, is a disease beginning by irritation, congestion, or chronic inflammation of the lower cervical ganglia of the sympathetic, giving rise at first to increased action of the heart.

The irritation or inflammation gives rise to hypertrophy of interstitial connective tissue of the ganglia, and consequent atrophy of part of the nervous element, with irritation of another part. Coincidently with, as sometimes happens, or subsequently, as is more usual, to the stimulation of the heart, occurs a sub-paralysis of portions of the vaso-motor fibres; hence congestion, followed by hypertrophy of the orbital connective tissue and of the thyroid gland.

That this assumption of coincident irritation and paralysis is not an arbitrary and entirely hypothetical one, may be shown by a consideration of progressive locomotor ataxy, where the characteristic lesion of the affected parts is nearly the same as in this case, namely, hypertrophy of interstitial connective tissue, with more or less complete destruction of nerve elements. It is well known that two of the most prominent symptoms are anæsthesia and severe shooting pains; both probably due to the same lesion interrupting the passage of normal secretions, and sending in their stead diseased ones. Anæsthesia is not unfrequently associated with neuralgia, and, what is more to the point, when the lesion is of a certain character, contraction of muscles may be associated with paralysis from cerebral lesions.

Exophthalmic goitre probably forms one of a group of nervous diseases, in which the connective tissue framework is hypertrophied at the expense of more important structures, to which it ought to be secondary. The symptoms of this group vary according to the portion of the nervous system attacked, rather than according to the nature of the essential lesion. They may be studied not only clinically, but, to a certain extent, in the light of physiology.

Among the distinct diseases belonging in this section are

some of those of the skin. In two of those mentioned nutrition can be said to be involved but slightly, if at all, and it is only from their connection with the others that they are spoken of in this place.

Many varieties of erythema are probably merely temporary paralyses of the vaso-motor cutaneous nerves, such as that singular form which occurs in the lower part of abdomen and upper part of thighs, preceding the development of variola; the region occupied by the erythema being spared by the subsequent disease; also erythema fugax, consisting of circumscribed patches without definite cause, or coexisting with gastric derangement. The tâche cérébrale of Trousseau, which he has observed in cases of exophthalmic goitre, is an affection of this character, but less permanent, consisting in a short dilatation and contraction of cutaneous vessels. I have seen small circumscribed patches of redness come and go within a very few minutes on the cheeks of a girl, with probably tubercular meningitis.

The more permanent forms, as, for instance, erythema nodosum, though undoubtedly involving, like all inflammations, a certain amount of vaso-motor disturbance, can hardly be considered to be essentially or principally vaso-motor neuroses.

When a smart blow is received on the skin, especially from a rod or lash, the character of the mark or wheal is well known, and serves as a sort of type to which some skin diseases are compared. It is somewhat raised, and usually white, with a pink border.

As the cutaneous muscles are of the smooth variety, and show the slowness of contraction characteristic of such muscles, it is not unreasonable to assume that they receive their nerves from the sympathetic system. The wheals are probably caused by a contraction of the skin muscles, with a relaxation of the vascular muscles on the border.

Dr. Gull * says: "If a line be traced with slight force on a skin which is prone to this form of contraction, the first noticeable change is a wrinkling of the surface, as in cutis anserina. In forty seconds there is a slightly raised line; in sixty seconds

* Hillier on Diseases of the Skin, Amer. ed., 1865, p. 58.

the line is palpably raised and hard; in ninety seconds there is an obvious wheal, which becomes fully developed in three minutes.

"If two points be marked on the skin previous to the friction, they are found nearer together after the wheal has risen. After dropping chloroform on the skin, however susceptible it might have been before, no wheal could be brought out by friction, and when chloroform was applied to a wheal already risen, it quickly reduced it."

Wilson * has sometimes noticed in white wheals of urticaria an alternate contraction and relaxation of the muscular structure, which gave the appearance of pulsation or of an ebb and flow of blood in the capillary vessels.

The nervous character of the disease is shown, beside the nature of the contraction on which it depends, by its causes, arising from mental emotions; gastric disturbance, especially from particular articles of food, such as shell-fish, or strawberries; and from certain drugs.

Hebra † mentions urticaria (also pemphigus), hysterica, with fibroid, carcinoma, or polypus of uterus, also in pregnancy or confinement.

It is not always unattended with nutritive changes, but occasionally gives rise to œdema, or is associated with vesicles or bullæ.‡

Acne Rosacea has been regarded § as a consequence of paralysis of the vaso-motor fibres accompanying the ethmoidal branch of the fifth pair, which follows a somewhat remarkable course through the ethmoid bone, to be distributed to the tip of the nose. If the disease goes beyond this locality, of course other branches of the fifth are involved.

The usual causes are either an excessive use of spirituous drinks, or a reflex disturbance from the female sexual organs.

It consists at first in a dilatation of the small vessels, which becomes more and more permanent, until a state of constant congestion is induced, and as secondary phenomena an hyper-

* Diseases of the Skin, London, 1867.
† MS. Notes.
‡ Hillier, op. cit.
§ Eulenburg and Landois, Wien. Med. Woch., No. 75.

trophy of connective tissue, and inflammation of sebaceous follicles.

"The influence of alcohol on the vaso-motor nerves is entirely indisputable. It can be observed in drinkers during the use of liquors, how in the face at first scattered red spots appear, which gradually increase in number and size, until finally a uniform dark red covers the whole countenance." "That by a repeated free use of liquor this temporary condition can become habitual, so that finally the vascular walls undergo a permanent paralysis, is evident." *

Hebra has called attention to the diagnosis between the different forms of redness, according to the stimulant habitually used, remarking upon the darker and colder nose ("kalt wie eine Hundsnase.") of the brandy-drinker, as compared with the countenance of the user of wine or beer.

From what has been many times said of the effects of simple vaso-motor paralysis, it is evident that this cannot be the only condition when the affected portion is colder than natural.

Herpes is a disease which is now quite generally conceded to be a neurosis, especially when occurring in the form of zoster.

Its frequent connection with neuralgia, its following the course and distribution of nerve-trunks, and its usual (though not invariable) unilateral occurrence, are the symptoms which give to it this character. Zoster, like a multitude of other diseases, is said to be caused by sudden emotion, especially anger.

Charcot † reports two cases of herpes, one in the course of a sciatic neuralgia, the other accompanying a traumatic neuralgia which followed a gunshot wound.

Brown-Séquard ‡ also reports a case where herpes occurred after cicatrization of a gunshot wound, in a place deprived of sensibility.

The same remark may be made of herpes as of many other diseases, that there is something more in its pathology than a pure vaso-motor paralysis.

There is little evidence in favor of that hypothesis of the

* Eulenburg and Landois, loc. cit.

† Journal de la Physiologie, vol. ii. p. 108.

‡ Ibid., Note.

production of herpes which supposes the nerves to be compressed in bony canals by the rapid dilatation of the arteries.

Charcot* reports a case in which an eruption resembling pemphigus was developed on the back of hand, after an injury to the nerve from incisions. Several incisions had been made for a boil, and one cicatrix, deep and adherent, corresponded to an important branch of radial nerve. There were also contraction of fingers, anæsthesia, and atrophy.

The anæsthesia had travelled up the arm above the situation of the wounds.

This case agrees in some particulars with those reported by Hutchinson, and before quoted, especially in the singular fact of the anæsthesia extending higher up the arm than the wound of nerve. Probably a similar explanation would apply to this case.

Beside the diseases just described, in which the disordered nutrition clearly manifests itself sooner or later through anatomically impaired structure, there are others in which the finer nutrition, that is, the changes accompanying functional activity, are deranged by vascular irregularities, depending on morbid innervation. Anatomical data are here less abundant, but their place is supplied by those derived from experiment.

This group might perhaps be philosophically arranged with diseases of secretion, since in each it is by the product that we detect the morbid condition.

It is designed here, however, to include principally the functional nervous diseases, where a local disturbance of the circulation will account most satisfactorily for the want or abnormal manifestation of functional activity. Some others will be mentioned where only a part of the symptoms come under this head.

The first and most important of these diseases is epilepsy.

The vaso-motor theory of this disease is of recent origin, but, beside furnishing the most complete explanation of the phenomena, rests at many points on a solid foundation of observation, though from the nature of the case not as yet supported by an unbroken series of facts.

The great variety of phenomena, which, although very different in many respects, may yet be grouped under this head,

* Journal de la Physiologie.

the intermittence of the attacks and constancy of the tendency, the usual absence of anatomical data, all lend probability to the theory of its being a vaso-motor neurosis.

The induration of medulla oblongata, observed by Van der Kolk, is probably a secondary lesion.

The paleness of the countenance immediately preceding an attack is probably the only visible one of the ordinary phenomena which would at first sight suggest that the vaso-motor nerves were implicated.

The character of the aura, whether felt or unfelt, and its relation to the paroxysm, relate to our subject only so far as to show the excitation of certain vaso-motor nerves to be in this case, as in most others, a reflex one.

Brown-Séquard * thus states the sequence of events in a complete epileptic seizure :—

Causes.	Effects.
1. Excitation of certain parts of the excito-motor side of the nervous centre.	1. Contraction of blood-vessels of the brain proper and of the face; spasm of some muscles of the eye and face.
2. Contraction of the blood-vessels of the brain proper.	2. Loss of consciousness and accumulation of blood in the base of the encephalon.
3. Extension of the first excitation, partly due to the accumulation of blood in the base of the encephalon.	3. Tonic contraction of the laryngeal, cervical, and thoracic expiratory muscles (laryngismus and trachelismus).
4. Contraction of laryngeal, and of thoracic expiratory muscles.	4. Crying and stoppage of respiration.
5. Farther extension of the first excitation of the nervous centre.	5. Tonic contraction extending to most of the muscles of trunk and limbs.
6. Loss of consciousness, and tonic contraction in trunk and limbs.	6. Falling.
7. Laryngismus, trachelismus, and the fixed state of the chest.	7. Asphyxia, with obstacles to the return of venous blood from the head and the spinal cavity.
8. Asphyxia and the accumulation of black blood in the encephalon, and in the spinal cord.	8. Clonic convulsions everywhere; contractions of the bowels, the bladder, the uterus; erection; increase of many secretions; efforts at inspiration.
9. Exhaustion of nervous power generally, and of the reflex faculty especially, except for respiration, which gradually becomes normal.	9. Cessation of the convulsions, coma or heavy sleep, after which extreme fatigue and headache.

* Phys. and Path. of Nervous Centres, p. 183.

It concerns us, so far as our present subject is involved, to know that cutting off the normal supply of blood from the cerebrum is capable of causing epileptiform phenomena, and that a spasm of central blood-vessels may take place from reflex irritation.

Kussmaul and Tenner,* in their experiments upon rabbits, found that any interruption in conveyance of arterial blood to the head produces epileptic fits as surely as hemorrhage does (except in weak or etherized animals). Compression of both carotids in the human subject may occasion loss of consciousness, dilatation of pupils, prolonged breathing, and general convulsions.

They found that, after ligature of subclavians (so as to cut off vertebrals) and one carotid, faradization of sympathetic on the other side turned the fundus of the eye pale and dilated the pupil, the neck being drawn backward, and violent convulsions in hind-legs taking place.

It is somewhat singular that compression of carotids, which Kussmaul and Tenner say may occasion loss of consciousness and convulsions, should be recommended as a preventive measure for epileptic paroxysms.†

The contraction of cerebral vessels takes place from reflex excitation. Nothnagel's ‡ experiments showed contraction of vessels in pia-mater on irritation of crural nerve. Partial interruption of vaso-motor nerves, as far as possible, diminished this effect. The fact that it could not be entirely prevented proves that some of the vaso-motor nerves of pia-mater take a course above even the superior cervical ganglion.

A remarkable case, as connecting clinical observation with experiment, is reported by Marowsky: §

"A cadet, *æt.* 16, had a moderate phlegmonous inflammation, extending from the left ala of the nose to the middle of the left cheek. The young man was naturally very excitable, nervous, but of strong build, and in good condition. The

* New Syd. Society's Translation.

† Flint's Practice, 2d ed., p. 691.

‡ Virchow's Archiv, xi. 203.

§ Deutsches Archiv für Klin. Med., vol. iii. p. 615, in St. Louis Med. and Surg. Journal, 1868, No. 5, p. 437.

tumor was very painful; at the circumference hard and red; the centre was dark-red, almost livid, and showed fluctuation so distinct as to indicate the propriety of opening the abscess. This little operation was performed immediately, the patient standing. A small but sufficient opening had scarcely been made with a sharp lancet, when the skin immediately around the incision, which had been dark-red, became perfectly white. This white margin rapidly extended farther and farther, all of the red inflamed surface became as white as chalk, the patient became restless, his pupils dilated, and his countenance grew pale; he fell, lost consciousness, and after a slight tetanic contraction was attacked with clonic convulsions of the face, hands, and feet, and foaming at the mouth. After a half, or, at most, one minute, the convulsions ceased; the patient became quiet; his face reddened; the pulse, which had been slow, contracted, and tense, became frequent and easily compressible; the skin moist; and after five or ten minutes, the patient awoke, remembered nothing of the accident, and for the rest of the day remained depressed, feeble, and incapable of mental exertion.

"The author supposes that he had caused, by the incision, a reflex spasm of the cerebral arteries, leading to general anæmia of the organ, which then caused convulsions and loss of consciousness. The tonic contraction of the cerebral vessels must have been general, affecting the base as well as the periphery, because loss of consciousness occurred simultaneously with general tonic and clonic convulsions."

The patient had no epileptic attack before nor since.

The facts above given show that for many cases of epilepsy the vaso-motor neurosis theory has considerable weight of evidence in its favor. Whether it applies to all cannot yet be determined. On the other hand, it is highly probable that many other functional nervous diseases have a somewhat similar pathological explanation. Brown-Séquard thinks that "various forms of insanity, of vertigo, of hallucinations, and of illusions; and also extasis, catalepsy, hysteria, chorea, hydrophobia, tetanus, local cramps, and even the general paralysis connected with insanity, may be due to irritations starting from a centripetal nerve."

As, however, the only data which connect these affections, or any of them, with a vaso-motor neurosis, are their more or less near relation to epilepsy, their starting from a centripetal irritation, and the known effects of general anæmia, any further description would be useless.

A single observation, from a distinguished source, leads us to give a place in this class of diseases to hemicrania.

Du Bois-Reymond * thinks that the hemicrania from which he suffers is a spasm of the arteries on the right side of the head, and under the control of the sympathetic.

At the time of the attacks the face is pale, the eye small and injected, pupil dilated. It is increased by everything which sends more blood to the head. His attention being directed to the cilio-spinal region, he found pressure to be painful. He considers the pain of the head to be owing to the pressure of the spasmodically contracted arteries on the nerves contained in their walls.

It is to be remarked of this explanation, first, that the symptoms agree neither with those of paralysis nor spasm of the sympathetic, since the injected eye appertains to an opposite condition from the pale countenance and dilated pupil, and an increase of blood sent to the head would hardly increase the pain if the arteries were in a state of spasm; and secondly, that Colin has shown that arteries are not very sensitive to ligature, and we might therefore say, *à fortiori*, to muscular spasm.

The circulatory phenomena are of course due to the intervention of vaso-motor nerves, but the chief trouble is quite as likely to be a neuralgia as a spasm.

Dr. Wilks † regards his hemicrania as due to paralysis of sympathetic.

Among the other phenomena which are to be explained in a manner analogous to that we have just described for epilepsy, those of the so-called reflex paralysis, and especially reflex paraplegia, have sometimes found a place.

This view of the affection is especially due to Brown-Séquard. He supposes that the peripheric irritation causes

* Journal de la Physiologie, v. 619.

† Med. Times and Gazette, Jan. 2, 1869; in Med. News and Library, Feb. 6, 1869.

contraction of the blood-vessels supplying the spinal cord, and consequent anæmia. The nerve-cells, being thus deprived of the normal nutriment, fail to act, and paralysis results. Brown-Séquard's statements of having observed such a contraction have been considered as reliable, until their correctness was questioned by Gull.*

After adverting to several forms of pseudo-paraplegia, he speaks of Brown-Séquard's assertion that he has seen "a contraction of blood-vessels in spinal cord (vessels of pia-mater) taking place under his eye when a tightened ligature was applied on hilus of kidney, irritating renal nerves, or when a similar operation was performed on the blood-vessels and nerves of supra-renal capsules. Generally the contraction is much more evident on the side of cord corresponding to irritated nerves."

Gull says there are no vessels visible on the spinal cord, except the dorsal vein and its tributaries; the columns of the cord being not much more vascular than tendons. The vessels are so distributed as to make it almost impossible that only one side should contract.

Other observers have obtained negative results from similar experiments.†

One argument in favor of some functional theory of the production of paraplegia from various peripheric irritations, consisted in the absence (as was supposed) of anatomical lesions. Modern methods of microscopic research have, however, shown that this argument does not apply to a very large portion of the cases.

Echeverria ‡ demonstrated anatomical lesions in the spinal cord in many cases of paraplegia, showing that in functional hemiplegia, so called, there is really organic disease.

It is probable, however, that there are cases, after making the exceptions claimed by Gull, Echeverria, and others, which are really functional, and which disappear with the disappearance of causative lesion.

* Guy's Hosp. Reports, 1861.

† Aitken's Sci. and Pract. of Med. Addition by Amer. Ed., vol. ii. p. 508.

‡ Reflex Paralysis, 1866.

Such are the cases of Mitchell, Morehouse, and Keen.* The theory of Brown-Séquard does not appear quite satisfactory to these authors, chiefly because spasm of vessels is well known to be usually followed by dilatation. They therefore suppose that the nutritive changes in cord are due rather to secondary congestion than to primary anæmia. They suggest, however, another hypothesis which seems to the author more plausible.

"If now we ask ourselves the question, whether it may be possible to blight or exhaust utterly the power of a nerve centre without the intervening mechanism of contracted or dilated blood-vessels, we are tempted to think that such a result may be producible."

"It appears to us possible that a very severe injury of a part may be competent so to exhaust the irritability of the nerve centres as to give rise to loss of function, which might prove more or less permanent." "The central change thus brought about would no doubt involve the consequent or immediate occurrence of chemical nutritive changes, which would gradually yield as time went on."

Echeverria, while rejecting Brown-Séquard's theory, attributes to the sympathetic a considerable share in the disease. It is not so clear, however, what he considers to be the nature of the connection.

The irregularities of circulation occurring with reflex paralysis, show only that the vaso-motor nerves are, like the others, liable to reflex paralysis or spasm. There seems no necessity for their intervention between the sensitive and motor nerves, in cases where the latter are paralyzed, and no reason why irritation should not produce in voluntary motor fibres the same effects,—whether spasm, as is more usual, or paralysis —that it has frequently been shown to do in the sympathetic.

These disorders, affecting various portions of the nervous system, do not exhaust the number of diseases which have been supposed to depend more or less directly on a sympathetic neurosis.

The ingenious views of Chapman as to the causation of cholera, diarrhœa, and sea-sickness, can hardly claim to be more than plausible hypotheses; and though perhaps leading

* Circular No. 6. Reflex Paralysis. Surgeon-General's Office, March 10, 1864.

to important practical results, can have but little scientific value. That disturbances of the circulation are involved in these, as in most diseases, and that these disturbances take place through the vaso-motor nerves, is well known; but that the latter hold any causative relation to the essential phenomena is purely a matter of conjecture.

SECTION VII.

THE spasms and paralyses which occur in the intestinal tracts present few points which can increase our knowledge of the pathology of the sympathetic. Spasm is concerned in colic, and perhaps is the essential condition. Other irregular and morbid contractions may take place, for instance, in intussusception, but we know of nothing in the mode of their occurrence which makes them interesting in our point of view. Peristaltic action is often exaggerated. It may be so from excitations transmitted from a distance, especially emotional. It is also increased in cases of obstruction from various causes. On the other hand, it may be diminished, and lead to important consequences from the constipation which results. This condition is constitutional, or may more often result from diversion of nervous energy in other directions.

Intestinal paralysis is, according to Romberg,* the cause of meteorismus or tympanitis. It is usually of secondary occurrence, and occasionally forms a prominent symptom in hysteria. Romberg says that diseases of brain and spinal cord are not followed by paralysis of the intestines.

The constipation and distention of the abdomen often accompanying injuries, especially of the spinal cord, depend upon paralysis of the abdominal muscles.

SECTION VIII.

THERE is even less to be said of the want of action on the part of the uterus, or the irregular contractions of various portions, sometimes so troublesome to the obstetrician. Undoubtedly, a morbid condition of the sympathetic branches supplied to the organ may be the cause, but there is, so far as the writer is aware, nothing further known in regard to its pathological action.

* Nervous Diseases, ii. 397.

SECTION IX.

THE pathological anatomy of the sympathetic system is scanty. The principal change is found in a hypertrophy of interstitial connective tissue, with consequent atrophy of nervous elements.

The nerves of this system may be affected with neuralgia. (Colic, cœliac neuralgia, perhaps angina pectoris.)

The iris often betrays irritation or paralysis of sympathetic from pressure by tumors of various kinds. It is also acted upon by lesions of cilio-spinal centres, and by reflex irritations.

It is for these reasons, and not from any essential connection, that progressive locomotor ataxy is sometimes accompanied by symptoms on the part of the pupil.

Many cases occur in which the effects on the temperature of vaso-motor paralysis, depending upon lesions either of nerve-trunks of spinal cord, or of encephalon, coincide with those determined by experiment.

The cases of diminished temperature depend either on atrophy (hemiplegia and some of paraplegia); on a circulation which, although when taken as a whole less impeded, is yet more liable to local interruption (paraplegia, perhaps some cases of injury to nerve-trunks); or on an irregularity of dilatation, together with contraction of vessels (injuries to nerve-trunks).

The phenomena of fever are largely dependent on sympathetic disturbance.

The secretions are influenced by the nervous system, principally by the action of the vaso-motor nervous fibres on the vessels. Certain motor nerves act apparently as stimulants to secretion, probably by their paralyzing effect on the sympathetic, possibly by a direct action on the secreting cells.

This effect is seen on the lachrymal, cutaneous, salivary, and mucous secretions, and probably on those of intestinal canal. The secretion or excretion of bile, and the production of sugar by the liver, are largely influenced by the nervous system.

Many affections of nutrition are caused by injuries to nerves. It is probable that the larger part are due to irritation, rather than to paralysis. A certain small amount of hypertrophy, (exophthalmic goitre, desquamation of epidermis) and sloughing are the most marked consequences of vaso-motor paralysis. Sloughing and ulceration, perhaps inflammation also, in some cases, sometimes follow irritation.

These are partly the results of disturbances of the circulation, and partly of anæsthesia and muscular paralysis.

The grounds for assuming the existence of "trophic" nerves are very slight.

Several diseases of the skin are connected with nervous disorder, probably of vaso-motor nerves (erythema, acne, urticaria, herpes, and perhaps pemphigus).

Some functional diseases are due to temporary disturbance of supply of blood, especially to central nervous system (epilepsy and allied affections).

The heart may receive too much or too little acceleratory stimulus from the sympathetic, and the same may be said of the intestines and the uterus.

Compliments of the Author

SOME CLINICAL OBSERVATIONS

ON THE

MALIGNANT DISEASES OF THE UTERUS.

By FORDYCE BARKER, M.D.

READ BEFORE THE ACADEMY OF MEDICINE, FEBRUARY 17, 1870.

In the remarks which I am to offer this evening, I shall not discuss the pathology of the malignant diseases of the uterus. I can add nothing, on this point, of value to the papers of Drs. Noeggerath and Kammèrer, recently read before the Academy; and the admirable analysis and *résumé* by Prof. William T. Lusk, in the September number of the *New York Medical Journal*, brings the subject fully up to the present state of science. I shall confine myself, therefore, to a consideration of the clinical features of these diseases.

Frequency of these Diseases.—It is generally asserted that these diseases are more frequent in cities than in the country; but this has not been demonstrated by any statistics of value, and Walsh,* basing his opinion on an analysis of the tables of Mr. Farr, Registrar-General of England, says: "Hence it appears that a town-life is far from exercising any serious influence on the production and mortality of cancer."

From the tables of the Registrar-General, Sir James Simpson† estimates the deaths in England (exclusive of London) from cancer of the uterus as about 3,000, from 1838 to 1842. M. Tanchou‡ found from the civil registers of the Depart-

* Walsh on Cancer, p. 111. Boston. 1844.

† On Diseases of Women, p. 42. Philadelphia. 1863.

‡ Recherches sur le traitement médical des tumeurs du sein. Paris. 1844.

ment of the Seine that the deaths in this Department from cancer of the uterus were as follows:—

In 1830	351
" 1831	391
" 1832	396
" 1833	398
" 1834	436
" 1835	508
Total in six years	2,480

My friend, Dr. Harris, Sanitary Superintendent of the Metropolitan Board of Health, has kindly furnished for me, from the Mortuary Records of the City of New York, statistics showing the amount of mortality from malignant and other organic diseases of the uterus and ovaries, from 1804 to October 1, 1869, 65¾ years. I shall append the tables and a letter from Dr. Harris to this paper. From the tables it will be seen that the reported deaths from cancer of the uterus during the ten years ending December 31, 1865, were 431. The deaths during 1866, 1867, 1868, and the first nine months of 1869, were 298. The yearly average of deaths from cancer of the uterus in this city during the last 3¾ years has been 79, while the average in the previous ten years was only 43 per annum. Thus, in fourteen years, there have been 729 deaths in this city from cancer of the uterus.

In fifty-one years, that is, from 1804 to 1855, the registered deaths from cancer of the uterus in the city of New York were only 247; but no case of death from cancer of the uterus was registered until 1837, and 247 would then represent the number of recorded deaths from this cause in 18 years, or an annual average of little more than 13. These statistics would seem to show that this disease is rapidly on the increase in this city. In a note to me Dr. Harris says: "It cannot be true that there has been such a tremendous increase in the number of fatal diseases of the womb, which are registered as cancer, as these three masses of the statistics would seem to indicate, even with liberal allowance for rapid increase of population. Permit me, therefore, to call attention to the fact that, since the Metropolitan Board

of Health organized improved methods of inquiry and registry, we record no deaths from asthenia, debility, shocks, hemorrhage, unexplained tumor, and other indefinite terms. Hence we find some increase in the accurately diagnosticated cancers of uterus, and a like increase in several other organic and dyscrasic causes of death." Dr. Harris adds: "Yet I must confess that, after a very careful analysis of the variable elements in the mortality records of the past sixty-five years, I am compelled to believe that malignant diseases of the uterus and ovaries are increasing far more rapidly than other dyscrasiac and organic causes of death in women." It may be true, as Dr Harris supposes, that cancer of the uterus is becoming more frequent in this city, but I should place very little reliance on these statistics as proof of the fact. How often is the question put to every practising physician, "Is not Bright's disease a great deal more common than in former times?" I have no doubt that the mortuary records of the city of New York would show a very much more rapid increase of this disease during the last thirty years than of cancer of the uterus, and for similar reasons, the deaths from Bright's disease having been formerly reported as resulting from apoplexy, convulsions, dropsy, etc. It needs no argument to prove that progress in the facility and certainty of diagnosis demonstrates the more frequent existence of a disease.

I think every one must be struck with the remarkable contrast between the mortality from cancer of the uterus in Paris and in New York, the population of Paris, from 1830 to 1835, being about the same as that of New York from 1865 to 1870; the average annual mortality from this cause being in one city 413, and in the other 79. This wonderful difference cannot be explained by any marked difference in the climate, habits, or morals of the two cities, or any other known cause, so far as we are acquainted with the laws of development of cancer. Is it possible that the great mass of the profession in one city is so much in advance in the diagnosis of the disease, as compared with the profession in the other city, as to explain the difference? I cannot regard this as probable. Is it a question of race? Cancer is said to be especially a disease of civilization, and more frequent in the cities than in the country. But this has not been proven by accurate statistics, and my own observation and

study of the subject would lead me to hesitate in accepting these assertions. Cancer is also said to be much more frequent in temperate than in warm climates, but I cannot find that this belief rests on any definite statistical foundation.

Comparative Exemption of the African Race.—I have recently learned some facts which seem to show that cancer of the uterus is comparatively rare in the African race. This idea was first suggested to me by my friend, Dr. Whitall, Resident-Physician of the Colored Home in this city. In the five years that he has been connected with the Home, during which time about two thousand colored women have been under his immediate observation, he has seen malignant disease of the uterus but twice.*

I also learn from my friends, Dr. J. C. Nott and Dr. Marion Sims, both of whom have had large opportunities for observation, that this disease is met with much less frequently "among the blacks than in the whites," and this opinion is confirmed by some interesting statistics kindly obtained for me by Dr. Nott, in a letter from Professor Chisolm, late of Charleston, now of Baltimore.

Etiology.—I think that the belief is very general, both with the profession and the public, that cancer is eminently an hereditary disease. Early in my professional life my attention was strongly directed to this point by the pathetic question put to me by a great sufferer from uterine cancer, "Are my daughters likely to go through with the same terrible experience?" I have often since that time been called upon to answer questions of similar import. Nearly all of the recent writers on cancer of the uterus give marked prominence to hereditary influence as a predisposing cause of the disease.

The attempt to establish general laws in pathology from statistics is extremely fallacious, except when based on very large numbers, and unfortunately, up to the present time, the statistics accumulated on this point are too few to warrant a conclusive decision. Lebert† found evidence of hereditary tendency in 14 cases out of 102. Paget‡ found in 322 cancerous

* Vid. appendix. Letters of Drs. Whitall, Nott, Chisolm, and Gilmore.

† Maladies Cancereuses, p. 218.

‡ Surg. Path. 3d Am. Ed., p. 674.

patients there were 78, or nearly one-fourth, who were aware of cancer in other members of their families. Sibley,* 33 in 305 cases.

Lebert,	14 in 102,	or 13.7 per cent.,	1 in 7
Paget,	78 in 322,	or 24.2 per cent.,	1 in 4
Sibley,	33 in 305,	or 8.5 per cent.,	1 in 9

Statistics bearing on this question in connection with uterine cancer are much smaller in numbers. Since September, 1853, I have recorded the result of my inquiry on this point in every case of malignant disease of the uterus that has come under my observation. I should say that a large majority of these cases from which my tables are made up I have seen and examined but once. Hereditary influence was noted whenever any relative of a former generation, as either parent, uncle or aunt, grand-parent, or brother or sister of grand-parent, was known to have had malignant disease in any form. It is possible that it may have occurred in some relative, when the fact was not known to the patient; but this would not seem very probable in a disease of such a marked and fearful character as this. I have found, in 487 cases of uterine cancer, some relative of a former generation reported to have had some form of malignant disease, in 36. In cancer of the uterus, a comparison on this point with the statistics of others will stand as follows:—

Lebert,	2 in 13,	or 1 in 6.5.	15.4 per cent.
West,	7 in 44,	or 1 in 6.2.	15.9 per cent.
Sibley,	8 in 135,	or 1 in 16.8.	5.9 per cent.
Scanzoni,	9 in 34,	or 1 in 3.8.	26.4 per cent.
Barker,	36 in 487,	or 1 in 13.5.	7.3 per cent.

If these statistics prove anything, they seem to show that the disease is hereditary in but a very small minority of cases. In fact they do not prove it to be hereditary at all. A constitutional tendency to the development of the disease—a cancerous diathesis—undoubtedly often exists in those whose ancestry have been entirely exempt from the disease.

In one family bearing an historical name, in which neither cancer nor phthisis has been known to occur in any branch of

* Medico-Chirurg. Trans., vol. 42.

the family in a former generation, either on the father's or the mother's side, the father and the mother now living at nearly eighty years of age, I have attended two sisters who died from cancer of the breast; and from a third sister, who is living, apparently in excellent health, I removed, three years ago, a cauliflower excrescence of the os uteri. I have now under my care a lady with cancer of the uterus, the second wife of a gentleman whose first wife, a sister of the second, died from the same disease sixteen years ago. Another sister has at the present time a very suspicious lump in the breast. The father and mother of these ladies are now living, and no member of the family in a former generation has been known to die from cancer. Two aunts have died from phthisis. Now, in both of these families there is manifestly a constitutional predisposition to cancer, but the disease was not inherited, as it did not exist in former generations.

In my examination of patients I was struck with the number who reported that consumption was in the family, and since 1860 I have kept a record on this point also. In 372 cases of uterine cancer, phthisis was reported to exist in the families of 92. It is only recently that I have been able to obtain, through the kindness of Dr. Purple, the volume of the Medico-Chirurgical Transactions containing the statistics of Mr. Sibley, gathered from the cancer records of Middlesex Hospital, London, of cases treated during the years 1853, '54, '55, and '56. Mr. Sibley says: "It is noted in 130 cases whether phthisis existed or not in the family of the patient. The question was answered in the affirmative in 48 instances. Comparing the statistics, they will stand as follows: Phthisis has existed in the family of those having cancer—

Sibley, 48 cases in 130. 1 in 2.7. 36.9 per cent.

Barker, 92 cases in 372. 1 in 4, nearly. 24.7 per cent.

Now, to my mind, all that these statistics prove is, that a vitiated constitution—a diathesis, whether it be inherited or acquired, whether it be tuberculous or cancerous, may predispose to the development of cancer. All the other causes which authors have assigned as predisposing to this disease, amount to just this and no more. I should say, then, nothing is known in regard to the *etiology* of this disease, as science up to the present

time utterly fails to explain what are the causes of this special form of heteromorphous development. I shall add, there is no evidence that the *disease* is, in any proper use of the term, *hereditary*. The *diathesis* is, undoubtedly, often inherited. But it does not follow that the child of a parent who has died from cancer necessarily or even generally inherits the diathesis; for both the diathesis and the disease of the parent may have been acquired subsequent to the conception and the birth of the child.*

Classification of Malignant Diseases of the Uterus.—I must again refer to the paper of Dr. Lusk,† as giving the most complete *résumé* of the present state of science on this point. Formerly scirrhus was regarded as the most frequent form in which this disease is met with in the uterus; but it is now known to be one of the rarest. The colloid form is also very rare. I have seen one only in which the character was determined by an autopsy and an examination of the diseased

* The *Biennial Retrospect of Medicine and Surgery*, for 1865-6, published by the New Sydenham Society (p. 228), refers to a paper in the *Brit. Med. Journal*, Aug. 26, 1865, "On the Antecedents of Cancer," by Mr. Moore. "Mr. Moore questions the hereditary nature of cancer. He does not deny to parental influence some effect in its production, but he adduces facts to prove that hereditary transmission is rarely noticed compared to the total number of cases; a direct transmission of the disease from parent to child being traceable only about thrice in a hundred cases. The bearing of the facts of both similar and dissimilar cancers occurring in parents and offspring is discussed; and, as an argument against the hereditary theory, it is noticed that cancer is least common at the period of life when parental influence is most active—*i. e.* in infancy. So, also, in infants born while the mother is suffering from cancer, the disease is not transmitted, nor, in families born from a cancerous mother, is it the younger children who are peculiarly liable.

"It usually attacks persons who have been all their lives in robust health; and if death happens from any accidental cause in the early period of cancer, all the organs of the body are generally found healthy; and at a later period of the disease the secondary deposits are found scattered in organs otherwise quite healthy. Cancer patients are also very commonly born of parents who have been very healthy and lived to extreme old age. A table is given to show that it is often observed that cancer occurs in the earliest children of large families, who are personally the most vigorous. On the whole, Mr. Moore concludes that there is no proof of a more peculiar hereditary nature in cancer than in other affections, such as malformations, etc., which are obviously not constitutional."

† Vide N. Y. Medical Journal, Sept., 1869.

structure with the microscope. In another case, which I saw with Dr. Linsly and Dr. Buck, I feel a strong conviction that the colloid form of disease existed, but there was no opportunity for verification of the diagnosis by an autopsic and microscopic examination. I have always accepted the distinction which has been made between cancer and cancroid disease of the uterus, and my record of the comparative frequency of these forms—the diagnosis being based entirely on the clinical aspect of these cases—is as follows:—

Cancer of the uterus	487
Cauliflower excrescence	18
Corroding ulcer	6
Recurrent fibroid	3

I am not yet prepared to accept the statement, that the "idea of basing a distinction between cancer and cancroid in the uterus upon differences of malignancy is wholly erroneous." It seems to me a very important fact, which both Klob and Paget have asserted, that cauliflower excrescence may exist primarily for some time as a harmless papillary growth, and subsequently assume the characteristics of malignancy. I think, clinically speaking, there is a great difference as to the comparative malignancy of the two forms; the malignancy of the one being wholly local for a time, and very slowly poisoning the general system; while that of the other very rapidly infects the whole system. I think the proof is now abundant and unquestionable that the locally malignant form, or cancroid, is often extirpated, and when wholly removed does not recur. But in cancer, this fortunate result occurs so very rarely as to be purely exceptional. I shall again refer to this point in connection with the subject of treatment.

Symptoms.—The symptoms occurring in malignant diseases of the uterus have been so fully described by authors, that I should be needlessly occupying the time of the Academy in giving a *résumé* of them. I propose merely to offer a few observations in regard to them. Vaginal discharges, pain, and hemorrhage are the three most prominent and characteristic symptoms. But it may be stated, as a general rule, that the symptoms of the earlier stages of the disease are so slight as

rarely to attract the attention of the patient, or at least not sufficient to lead her to seek medical aid until the disease has made considerable progress, unless some accidental circumstance may have led to a physical examination. In one patient the first symptom which attracted attention was the fact that the husband was alarmed by finding that an herpetic eruption on the penis always followed coition. This patient was unaware of the existence of any leucorrhœal discharge, and had never suffered from pain or hemorrhage. Another patient, aged 52, who had not menstruated for nine years, and who was entirely free from all symptoms of disease that she was aware of, was alarmed by observing that coition, which was not attended with pain, was always followed by a slight discharge of blood from the vagina. A third, who, as her husband informed me, had been cold and passionless to an unpleasant degree during menstrual life, three years after the climacteric, began to manifest a most extraordinary sexual appetite. It finally became so serious a matter for the husband, and so much a source of religious sorrow for the wife, that I was consulted. This patient had the appearance of perfect health, and she could not complain of a single symptom, except the one mentioned. But in this case, as in the other two before referred to, a digital examination revealed the existence of advanced carcinoma.

The vaginal discharges are, no doubt, one of the first symptoms that attracts the attention of a majority of patients. The characters of these discharges are so well known that it is needless for me to allude to them, except to say that none of them are pathognomonic of malignant disease, the various fetid and sanguinolent discharges often occurring in non-malignant diseases of the uterus. Neither is there anything peculiar or characteristic in the odor of the discharges. The odor resulting from sphacelation of part of a fibroid, from strangulation of a polypus, from decomposition of a portion of retained ovum, and more emphatically from retention and decomposition of the secretions within the cavity of the uterus by the compressed sponge, is oftentimes quite as offensive and insupportable as the discharges from malignant diseases. Most authors agree with Becquerel in describing the pain of cancer as being very sharp, intense, and lancinating, and not influenced by loco-

motion or movement of any kind. In my experience, pain very rarely occurs as a prominent symptom, until the disease is far advanced; and in some cases, throughout the whole course of the disease, it never amounts to more than a general sense of *malaise* or pain in micturition or in defecation. In some cases there are frequently-recurring flashes of pain darting from the pelvic region, but undefined as to locality, which often awaken the patient from sound sleep. In a few cases, but in a *very small* minority, the pains in the advanced stages have been most atrocious, producing as intense agony as I have been called upon to see the human system endure.

In quite a number of cases I have been struck with the absence of all appearance of cachexiæ up to the time of death. In some of these cases death has resulted from hemorrhage, as in a patient of Dr. Thebaud, who died from this cause away from the city, three years after the first manifestation of the disease. In others the death has resulted from secondary peritonitis, perhaps before the disease has poisoned the general system, by decomposition and septicæmic absorption.

Peritonitis occurs in a very large proportion of cases during the progress of the disease. In most cases that I have seen, the attacks have been partial and recurrent, but in some few the peritonitis has been general and the immediate cause of death. Congestion and inflammation of the urethra, bladder, and rectum, with their concomitant symptoms, are ordinarily developed in the advanced stages of the disease, even when the cancerous deposit does not extend to these organs. So, also, from this cause, in many cases, the ureters become partially or completely obstructed, producing partial or complete suppression of the urine. In the paper of Dr. Lusk, which I have before alluded to, he quotes Cornil as saying: "With these obstructions to the excretion of urine, we expected uræmic symptoms to be manifested, but they were not. Although the attention of M. Charot and myself was fixed upon this point, we never saw either the epileptiform convulsion or the coma peculiar to uræmic poisoning." Dr. Lusk also says that "Seyfert has been in the habit of employing this fact for years as an argument against the uræmic theory of eclampsia." In my experience two instances have occurred which do not coincide with the above statements.

In 1853 I several times saw, with the late Dr. Henry G. Cox, a patient in whom the cancerous deposit had extended from the uterus to the vesico-vaginal and recto-vaginal walls. As I lived much nearer to the patient than Dr. Cox, I was called in the night to see her on account of an attack of convulsions, and found that no urine had been passed for nearly forty-eight hours. I introduced the catheter, but found no water in the bladder. During the night she had five convulsions. The tendency to convulsions seemed to be overcome by chloroform, and the next day the excretion of urine became abundant, and she lived more than a year after this occurrence.

In 1858 I saw several times, with Dr. Oliver White, a lady with extensive cancer involving the uterus and neighboring organs, who, during my attendance with Dr. White, had complete suppression of urine, accompanied with profound coma, which continued, as Dr. White informs me, nearly four days. In this case also I passed the catheter, but there was no water in the bladder. Suddenly a most enormous discharge of urine took place, and the patient gradually came out of the comatose state, and lived some weeks.

Diagnosis.—The diagnostic characters of the malignant diseases of the neck of the uterus are now so clearly given by authors, and so readily made out, that I have nothing to add on this point. I heartily concur with my friend, Prof. Thomas, in his statement that "for the proper differentiation of true malignant disease from neoplasms and the results of inflammation we are indebted to no one so much as to Dr. Henry Bennet of London."

But little, however, has been written on the diagnosis of uterine cancer where the cervix is not involved. In fact, until within a few years past, the cervix has been regarded as almost invariably the primary seat of the disease, and this belief has been sanctioned by the authority of Ramsbotham, Sir Charles Clarke, Walshe, and Rokitansky. But it is now known that it not unfrequently occurs primarily in the cavity or in the parenchyma of the body of the uterus. A very considerable number of cases of this kind has been reported. Sir James Simpson thinks that out of thirty cases of cancer of the uterus two are of this kind. I have met with several, but by no

means in so large a proportion as the estimate of Sir James Simpson. The primary seat of the disease may be either in the mucous membrane of the cavity or in the parenchyma of the body of the uterus. I will mention, as an illustration of the first form, the case of a lady in Williamsburg, from whom, in 1852, I removed a polypus which had its attachment in the cavity of the body of the uterus. She was then forty-five years of age, and from that time for six years she was in excellent health, having no vaginal discharge, either leucorrhœal or sanguineous. In the autumn of 1858 she again called upon me on account of a constant sanious discharge with a slightly fetid odor, and moderate hemorrhage, which had twice occurred. The cervix uteri appeared perfectly healthy to the touch, and presented no abnormal appearance on examination with the speculum. Some months subsequently she again called upon me, as she had had several attacks of rather severe hemorrhage. She looked sick; her countenance was pale and wrinkled, but she complained of no pain, only of weakness. I dilated the cervix by compressed sponge, when I could feel with my finger a small irregular mass, not larger than a filbert, which was easily torn by the nail, and bled profusely. I will not detail the various means that I resorted to to restrain and destroy this mass, as she died early in the summer of 1862. A recent case affords a most interesting example of that form of the disease where the primary seat of the cancerous deposit was in the parenchyma of the body of the uterus. A lady, mother of several children, ceased to menstruate in 1857, when she was forty-nine years of age. She had no vaginal discharge from this time until 1864, when she had a moderate hemorrhage from the uterus, which then caused some alarm. Since that time there has been a constant leucorrhœal discharge. For four years past, this discharge has been frequently, although not constantly, sanious and fetid. She has had several attacks of severe hemorrhage, and two or three attacks of peritonitis. She has been under the professional care of Dr. Griswold, of this city, and Dr. Edward Parker, of Poughkeepsie. During the last two years she was repeatedly seen and most carefully examined by Drs. Peaslee and Noeggerath, and more recently by myself. The cervix uteri seemed unaffected by disease up to the time of her

death, two weeks since. At the post-mortem examination made by Dr. Lusk—Drs. Griswold, Peaslee, and myself being present—a large amount of sero-purulent fluid was found in the peritoneal cavity, and in the region of the pelvis the intestines, bladder, and uterus were agglutinated in a mass difficult to separate or distinguish. The uterus was enlarged, and through the posterior wall there was a perforation by ulceration communicating with a circumscribed sac filled with pus, situated above the pubis, and extending down into the cul-de-sac of Douglas, where it again communicated with the cavity of the uterus, through an extensive loss of substance in the uterine walls, just above the junction of the cervix with the body. Dr. Francis Delafield has kindly made for me a microscopic examination of the uterus, and has given me the following report: "The cervix is of usual length and size, its internal surface rough. The body of the uterus is increased in thickness, its internal surface rough and ulcerated, its posterior wall partly destroyed and communicating with a cavity between the uterus and rectum. The mucous membrane of the body is entirely destroyed. Its roughened, internal surface is composed of irregular pieces of muscular tissue and new growth, both fatty and granular. Beneath this, the muscular tissue is replaced by a tissue composed of the normal muscle, mixed with alveoli containing cells, and with masses of small, round, lymphoid cells. The walls of the alveoli are composed of muscle, of fibrous tissue, and of lymphoid cells. The cells contained in the alveoli are small, polygonal, with one or two nuclei, and have an epithelial character. These cells are nowhere very closely packed together, nor do they present any of the so-called nests. The infiltration of lymphoid cells is everywhere very abundant. The muscle-cells are increased in size and easily isolated. The infiltration of new growths extends entirely through the posterior wall of the organ. In the walls of the cervix the same infiltration exists; but here the alveoli and epithelial cells are less abundant and the number of lymphoid cells greater. If we are to consider the place where the new growth is most advanced as the point where it originated, the posterior wall of the body of the uterus, just above the cervix, is the primary seat of the disease. The specimen

is, therefore, a well-marked example of carcinoma uteri. It differs in some respects from the most ordinary form of cancer of this organ. The epithelial cells are smaller, less closely packed, and present none of the usual pockets or nests; so that the new growth assumes the anatomical character rather of true than of epithelial cancer. Still, in view of the great irregularity of forms under which epithelioma, or cancroid exists in the uterus, it seems proper to call this specimen one of epithelioma of irregular type."

The diagnosis of malignant disease, when its primary seat is in the mucous membrane of the body, can generally be made out from the following symptoms, as summed up by Sir James Simpson:—(1) "The presence of a constant, profuse, and offensive discharge; (2) Frequent, profuse, and intractable menorrhagia; (3) The presence of a hard, rough, and irregular tumor in the cavity of the uterus, detected by the uterine sound and felt by the finger after the dilatation of the cervix; (4) Recognition under the microscope of the morbid structures, as evidence of cancer; (5) The periodic recurrence of pains, slight and intermittent, perhaps, at first, but soon reaching a high pitch of intensity, and then gradually passing off." But where the malignant disease is primarily interstitial in its seat the diagnosis becomes much more difficult. Dr. Grailly Hewitt mentions the following symptoms as the leading points of diagnosis: "The patient presents symptoms of uterine cancer—a vaginal examination fails to give evidence of it—a careful examination of the uterus above the pubes reveals the existence of an irregular, or possibly of a regular tumor, which may be of considerable size. This tumor, which in its physical character may resemble a fibrous tumor of the uterus, is distinguished from the latter by the presence of a more or less constant, offensive, and bloody discharge, and some of the constitutional symptoms of cancer." In cases where vaginal examination fails to detect disease of the cervix, I regard hemorrhage, coming on spontaneously some years after the climacteric, as almost conclusive evidence of malignant disease, for I have never known polypi or fibrous tumors to bleed, for the first time, any considerable period after menstruation has ceased.

Duration of the Disease.—The physician in attendance in a case of malignant disease of the uterus is constantly asked how long she may live. Relying upon the statements of authors on this point, my answer formerly was generally wrong, as I found that my patients almost invariably outlived my expectations. Lebert gives the average duration in 39 cases as a little over sixteen months. West, in 17 cases, found the average duration 15 months. Simpson says: "Patients usually die in from two to two years and a half." From personal observation I can give the duration from the time of the first symptoms of the disease to the time of death in only twenty-six cases, fifteen of which have been under my own charge, and the remaining eleven I have seen in consultation with others. The average duration in these 26 cases was three years and eight months. I cannot explain the difference between these results and those of the authors before quoted. I only know the facts to be true. In one case now living, it is eleven years since I first made the diagnosis of cancer of the uterus. She is still in such health that she visits her friends, attends church quite regularly, occasionally goes to the opera, and no one suspects the existence of the disease except her husband, myself, and an old nurse who has been with her since the birth of her first child, now 26 years of age. This nurse has acquired great dexterity in making applications to correct the fetor and restrain the leucorrhœal and hemorrhagic discharges. It is at least four years since the cervix was entirely destroyed, and more than a year since, in a vaginal examination, the finger passed into a large cavity, so that the walls of the uterus seem to be a mere shell. I have another patient still in a pretty good general condition, in whom I recognized the existence of cancer nearly five years since. I am glad to observe that Courty in a great measure confirms my experience on this point. He says: "I have very positively seen women succumb to the progress of uterine cancer several years, sometimes seven or eight, after the probable epoch when it had commenced to develop."

Treatment.—In the cancroid diseases, I think the fact has been established, beyond controversy, that the disease may be successfully treated by ablation, if the operation be performed sufficiently early to extirpate the entire diseased structure. In

the London *Medical Times and Gazette*, Oct., 1860, nearly ten years since, Mr. Hutchinson published a tabular statement of fourteen cases of "epithelial cancer of the female genitals," in which eleven recovered after excision. In two the disease recurred, and the patients died. In one the disease was too extensive for any operation. Since that publication, scattered through the medical periodicals, many other successful cases have been reported. Since 1856 I have ablated the cervix in eleven cases for this cause, nine of which have recovered and remain well. In one, death followed two days after the operation, from peritonitis, induced evidently by the imprudence of the patient. In the other, a patient of Dr. John W. Warner, the disease recurred a few months after, and the patient subsequently died.

In view of all these facts, it must be conceded that science has made great progress in successfully treating a class of diseases, which until quite recently invariably resulted in a fatal termination. But the same success cannot as yet be claimed as to the other forms of malignant diseases of the uterus. And yet I cannot but feel that there is much to encourage us to hope that in the future we may yet attain a very much greater success in the treatment of this most terrible disease, than as yet has been dreamed of by the most sanguine of scientific men. In the first place, we are making rapid progress in our knowledge of the histology of the disease, and the laws of its development. In the next place, facts are accumulating which show that spontaneous recovery has occurred in some cases, and art may yet, in a certain percentage, secure the same result. Aran quotes one case from Estevenet, in which the cancerous uterus was separated entirely from its attachments, and expelled by the woman in the effort of defecation, so that for some time there were hopes of her cure. Prof. Habit reports one case of spontaneous recovery in which the cancer involved both the uterus and the vagina. Virchow, Kiwish, Rokitansky and Scanzoni, each report one case of spontaneous recovery from cancer of the uterus. Two cases of recovery have occurred in my own experience, unless there was some mistake in the diagnosis. As both of these cases were of great interest to me, I shall detain the Academy by a somewhat detailed history of each.

The first, a married woman, aged 38, mother of four children, the youngest eight years of age, came to my clinic in 1852. She had previously been seen by the late Dr. Mott and had also been at the clinic of Professor Bedford, both of whom, as the patient said, told her that she had cancer of the womb. Whether this be true or not, I regarded it as such. She came to my clinic for nearly two years, during which time I applied the actual cautery some fifteen or sixteen times. I think, also, my friend, Prof. Charles Budd, who was then assisting me in my clinics, applied the cautery a few times. We were both of us greatly surprised at the result, as the disease seemed to manifestly decrease after each application, but it was not entirely gone, when she became pregnant. Dr. Budd attended her at the time of her confinement, when she gave birth to a living child. The first stage was completed before Dr. Budd saw her, and the second stage was less than an hour, a complete refutation of the *à priori* objection which has been urged against the actual cautery, viz., that it is liable to leave an indurated cicatrix, which would prevent dilatation of the cervix in the first stage of labor. Dr. Budd has informed me within a few months past that this patient is still alive and in good health. I know not whether, in the light of my accumulated experience since that time, I should regard that case as one of cancer or not, but I think there can be very little doubt as to the following:—a case which, for a time, brought upon me some unmerited reproach, but which has subsequently given me a great deal of undeserved reputation. In 1856, the wife of a prominent lawyer and politician in Massachusetts came to this city for consultation. She had previously been under the care of the late Dr. Green of Worcester, and been examined by one or two eminent physicians in Boston. Her husband informed me that they had called her disease "cancer of the womb." She was cachectic and very feeble, and for more than a year she had been suffering from severe and frequently recurring pains, repeated and profuse hemorrhage, with a constant, sanious, and extremely offensive discharge. The cervix was much enlarged, with a jagged, irregular surface, and the uterus was fixed and immovable in the pelvic cavity. My opinion as to the nature of the case coincided with that which had been

expressed by those who had seen her before, and, of course, my prognosis was most unfavorable. But with the hope of diminishing the profuse vaginal discharge and the extremely offensive odor, and that by accomplishing this I should make her journey home more comfortable, I decided to apply the acid nitrate of mercury freely to the ulcerated surface of the cervix.

Some two weeks afterwards, I received a letter of bitter reproach from her husband, in which he said that soon after her return, her mouth became very sore, and her physicians declared it to be the worst case of salivation that they had ever seen. Several of her teeth had come out, and the discharges from the vagina had increased enormously, with which she had thrown off masses of putrid and horribly offensive substances, so that it was difficult to remain in the room with her. The letter closed by saying: "Before you receive this my dear wife will have left me, and I shall always bear in my heart the feeling that you, by your experiments, have greatly added to her suffering and shortened her days." A few months afterwards I received a letter from him of quite a different tenor. She had quite recovered her health, and the disease was apparently cured. I have twice, within a few years, had the opportunity of making a vaginal examination of this lady. At the upper portion of the vagina a puckered cicatricial indentation can be felt and seen by the speculum, but nothing like the cervix or body of the uterus can be felt through the vaginal walls. Through the rectum I am able to detect what I suppose to be the remains of the body of the uterus. She is now 59 years of age, fat and stout and in robust health.* It is an old adage that converted enemies become the most zealous of friends.

* Since this paper was read before the Academy of Medicine, a case has been published by Dr. Mettauer, of Virginia, which, in all essential features, resembles in a most remarkable degree the one related above. As these cases are, I believe, unique in medical literature, no apology is necessary for giving an abstract of Dr. Mettauer's case. "The subject of this case was a negress, aged about 35, married, but had never conceived, and who had for years suffered from uterine irregularities. . . . When examined per vaginam, the os and cervix, as well as a large portion of the body of the uterus, had disappeared entirely, as they could not be discovered by the most careful examination by the *toucher*, and the chasm remaining would have readily received an egg of moderate size. The examination caused some hemorrhage as well as pain, and the woman represented that bleeding was of frequent occurrence.

If a young physician, called upon to take charge of a case of uterine cancer, should take up the most recent systematic works on gynæcology, as Byford, Thomas, Simpson, Hewitt, Courty, or Scanzoni, to ascertain what he could do to arrest the progress of the disease, to palliate symptoms, and to relieve suffering, I think the result would be a despondent impression as to any positive service he could render to his patient. Yet, in few diseases are the resources of art so manifest and so conclusive as in this, for we can most certainly improve nutrition, relieve pain, secure sleep, and arrest the sanious offensive discharges and hemorrhage, and thus notably prolong life and make it tolerable to the end. I have often thought that the feeling on the part of the physician that the case is hopeless discourages the constant and assiduous care which may really be of great service to the patient.

It seems to me that, within a few years past, our therapeutic resources have greatly increased, and that we may confidently

The health of the patient was greatly impaired, as emaciation and debility had nearly rendered her helpless. So appalling was the condition of this poor woman, that the writer, after a full examination, determined to discard the case, believing it utterly hopeless. The entreaties of the woman, however, induced him to alter his resolution, and, merely to quiet her distress, his consent to undertake to treat the case was yielded. After regulating the bowels, and the use for several days of soothing vaginal injections, it was determined to use the acid nitrate of mercury to the ulcerated uterus, which was applied by saturating cotton lint with it, and then conveying the tampon-like body thus medicated to the affected parts through the expanded three-bladed speculum. The lint was heavily charged with the nitrate, but not to supersaturation, and carefully pressed into the ulcer. Over this was laid a thick pledget of lint saturated with salt water, carefully packed round the first dossil. . . . . In three days violent salivation came on, which continued over a month, during which there was no examination of the vagina or the ulcer. The lint came away during this time, but it was not known when. . . . The condition of the uterus was examined into the fifth week after the acid nitrate was applied, and most unexpectedly was found to have healed entirely, but no os, cervix, or appreciable body could be discovered, and all discharge had ceased. From this time the general health rapidly improved, and in two months the woman was apparently restored to sound health, her recovery being greatly promoted by the daily use of iodide of iron and a generous diet. She lived four years after this severe trial, enjoying good health most of the time, but finally died of some acute disease, of the nature of which the writer has never been informed."—*Contributions to Uterine Surgery, by John P. Mettauer, M.D., LL.D., Virginia. From Bost. Med. and Surg. Journ., March* 10, 1870.

look in the future for additions of great value. I shall therefore presume to add a few words on the constitutional, the local, and the palliative treatment of this disease.

Constitutional Treatment.—The doctrine that cancer has a constitutional origin seems to have been accepted without question until quite recently; but it is now beginning to be doubted whether the disease has not a local origin which develops secondarily a blood contamination. This view has been put forth with great force by Mr. Moore, of Middlesex Hospital, London,* and I think that those who have studied the subject are strongly inclined to accept his doctrine. If in the future it prove to be true, we may anticipate great results and new discoveries of therapeutic agents in the constitutional treatment of this disease. I shall only allude to such as I can speak of from personal experience in their use.

Arsenic has long been in use in the treatment of cancer, and a host of prominent names might be quoted who have expressed great confidence in its value in this class of cases. But with the great majority of even those who have devoted their attention especially to this class of diseases this remedy does not seem to hold an accepted position as one possessing any specific value; for many years, however, I have used it, and have regarded it as more useful than any other known remedy in this class of cases. My experience has convinced me that it possesses a decided power in improving the general condition of the patient as to flesh and strength, in diminishing the discharges, in mitigating pain, and in arresting the progress of the disease. Patients themselves become so convinced of its usefulness, that I have never known one willing to give it up, after having commenced its use. I prescribe it in small doses, three drops of Fowler's solution, to be taken always after eating. I have not been in the habit, until recently, of suggesting its use in the very advanced and hopeless cases, because I have expected but slight, if any, benefit in such cases, and I did not wish to jeopardize the reputation of a remedy which I esteem so highly. But in the past two years, I have often suggested it in these cases, and I believe it has always proved serviceable.

* Brit. Med. Journ., Aug. 26, 1865.

The Missisquoi water of St. Alban's, Vermont, has in some cases seemed to be of great benefit in arresting for a time the progress of the disease. In many, the use of these waters has seemed to produce no results. Many others, to whom I have suggested their use, have never reported to me, and it is therefore probable that the results in these cases were negative. But in a few instances, one of cancer of the breast, and four of uterine cancer, the apparent effect has been more striking than any results that I have ever seen from any other agent. None were cured, but the ulcerated surface was cleaned off, as though it had been excised with the knife, the fetor, the discharge, and the pain ceased for some time, and the general health of the patient was greatly improved. One, a poor woman, whom I first saw in May, 1867, with ulcerated carcinoma, involving both the neck and the body of the uterus, and exceedingly feeble, emaciated, and broken down by the profuse and horribly offensive discharges and repeated hemorrhages, rapidly improved both in her local and general condition under the use of these waters. She is still alive, although the uterus is nearly destroyed, but her general condition is much better than when I first saw her. I will allude to another case, a patient of Dr. Sims. I first saw this lady with him in December, 1868. Dr. Sims informed me that, when he examined her in Paris, in August, the whole surface of the enlarged cervix was one mass of rough, ragged ulceration. When I examined her, the diseased surface was as smooth as if it had been excised with a knife, and there was no discharge and no odor, although she suffered from horrible pains. She died a few months subsequently, but Dr. Sims expresses the strong conviction (and I concur with him), that her life was prolonged by the use of the Missisquoi waters. Although the general sentiment of the profession does not coincide with mine on this point, I cannot but avow the belief that the therapeutic effects of these waters are eminently worthy of careful investigation.*

* In a letter which I have recently received from Dr. D. F. Fassett, of St. Alban's, Vermont, a gentleman who is probably more competent than any one else to give an opinion based on an extensive personal observation, as to the effects of these waters, he says: "I have notes of four cases only of malignant disease of the uterus, under my own observation, all of whom used the water,

Dr. F. A. Burrall * has published a very interesting case of apparent cure of ulcerated scirrhous cancer of the breast. It was for months treated with an external application of a watery solution of carbolic acid and glycerine, without arresting its progress; but it began to diminish in size and soon cicatrized, after the internal use of carbolic acid and the sulphate of quinine was added to the treatment. A tablespoonful of the solution of sulphate of quinine (four grains to the ounce of water), with three drops of the solution of carbolic acid (two drops being equivalent to one grain of the crystals), was taken three times a day. It seems to me that such cases as these are of some weight in sustaining the theory of Mr. Moore.

Local Treatment.—In all cases where the cancroid disease is confined to the cervix and can be wholly removed, I think the question is now settled that amputation of the cervix should be performed. Most operators use the *écraseur* for this purpose, but I have always used the knife, because I think it leaves the excised surface in a much better condition for healthy cicatrization, and I have never had any trouble from hemorrhage after the operation. I have several times seen the *écraseur* used for this purpose, and I cannot resist the feeling that the tissues must be more injured by this crushing process, and that they are left more liable to morbid development subsequently, than if the wound had been made by the clean cut of a knife. I am quite certain that the process of cicatrization is more rapid. It is hardly necessary for me to add that the operation should not be thought of, if the disease exist above the vaginal attachment of the cervix, or involve the mucous membrane of the vagina, or where there is any evidence of its existence in any other part of the body.

one with marked benefit, and three with no benefit. But I have seen many cases of malignant disease located elsewhere, as in the breast, face, and extremities, where the effect was to cause marked improvement in the general health and in the local manifestation of the disease, but there was no positive cure."

I may add that, in my observation, with one exception, none have received any apparent benefit from the use of these waters purchased in bottles. It has been only those who have taken the waters directly from the spring that have seemed to improve.

* New York Medical Record, Nov. 1, 1869.

Cauterizations.—There is a great difference of opinion among writers on this subject, as to the utility of cauterizations of the ulcerated surface of epithelioma. By some they are regarded as useless, if not positively injurious; as useless, because they do not reach and therefore cannot destroy the whole extent of the morbid tissue; and injurious, because they may influence deleteriously the adjacent structure and excite inflammation, thus favoring extension of the cancerous deposit and hastening the progress of the disease. I have carefully studied the objections which have been urged, and I admit their full force, if cauterizations be indiscriminately and carelessly made. But under certain circumstances I have the strong conviction that they are often very useful in arresting the ulcerative process; in diminishing the profuse discharges, and thus saving the strength of the patient; in correcting the fetor, and thus lessening the chances of septic absorption. Where cases are seen at an early stage, and the ulcerated surface is small in extent, and the subjacent induration not profound, the progress of the disease has seemed to be manifestly retarded by their use. The principal agents which I have used for this purpose in the malignant diseases of the uterus are the actual cautery, the acid nitrate of mercury, and the chromic acid. In those cases where the malignant deposit has not extended to the adjacent organs, and particularly to the mucous membrane of the vagina, I use the actual cautery far more frequently than any other agent. My reasons for this preference are the following: It is a much more manageable agent than any other, as it only acts upon the part with which it immediately comes in contact; the depth of cauterization can be perfectly regulated; no pain is produced by its use, either at the time or subsequently;*

* I have seen pain caused in the use of the cautery, by the nervous hurry of the operator, who pushed the iron with some force against the os, but never under any other circumstances. In a patient whom I saw twice with Dr. Thebaud he made use of Nelaton's gas-cautery, by which he was able to effectually cauterize a funnel-shaped cavity in the body of the uterus after the cervix had been entirely destroyed by the disease; and the patient believed, as I believe, that the effect of each cauterization was decidedly useful, as she suffered very little during the progress of her disease from profuse or offensive discharges, or pain. She lived about two years and a half after her

and when the slough is thrown off no hemorrhage follows, as I have often seen occur when other agents are used.

I have chiefly used the acid nitrate of mercury in those cases where granulations sprout from the interior of the cervix, which have a great tendency to bleed. But for some years past I have become very cautious in its use, as in three instances I have caused profuse salivation by one application of this agent, and I have heard from medical friends of several other cases of like result. The susceptibility to this effect seems to be manifestly increased in a broken-down, cachectic system, and in such it is a consequence greatly to be deplored.

In cauliflower excrescence, where the disease has extended beyond the reach of the knife, I generally make use of the chromic acid (100 grains to the ounce of water), applying it very carefully with a pencil brush to the surface. Ordinarily its action is not followed by pain, but I should mention that in one case, six hours after the application of the chromic acid, severe pain came on, so that I was sent for in the night, and, after using three hypodermic injections of morphia without relief, I was obliged to keep my patient under the full anæsthetic influence of chloroform for nearly four hours. A pretty severe attack of peritonitis followed, from which she entirely recovered in a few days. There was complete and apparently healthy cicatrization of the diseased surface, and for more than a year I deluded myself with the hope that the disease was cured. But when it again appeared its progress was very rapid, and she died in eight weeks after the first symptom of its recurrence. I have repeatedly made trial of various other caustics, such as the arsenical paste, chloride of zinc, etc., but without such results as lead me to substitute either of them for those above mentioned.

Dr. Routh,* of London, has published two very interesting cases of apparent cure of epithelial cancer of the cervix uteri by the use of a strong spirituous solution of bromine (bromine

disease was known to Dr. Thebaud, the immediate cause of her death being a sudden, violent hemorrhage, which occurred when out of town.

* On a New Mode of Treating Epithelial Cancer of the Cervix Uteri and its Cavity. By C. H. F. Routh, M.D., etc. Transactions of the Obstetrical Society of London, vol. viii., 1866, p. 290.

gr.v.—x. to fifty minims of spirits of wine). Since the publication of the paper by Dr. Routh, I have seen no case where the use of this agent would be admissible, but I am determined to try it when a proper case shall offer. It should be mentioned that Dr. Routh regards it as a powerful caustic, which is not unattended with danger to the patient if not carefully applied, and to the operator, in its preparation. I have made great use of bromine in the treatment of the malignant diseases of the uterus, but in a different way and for a different purpose, which I shall allude to presently.

Palliative Treatment.—On this point I shall add but a few words. I have long been impressed with the belief that many practitioners err in allowing the victims of this disease to suffer by trusting to feeble and inefficient agents, such as hyoscyamus, belladonna, cannabis indica, etc., for the relief of pain, from the fear that the patient might become habituated to the use of opium, in short, an opium-eater. Now, I hold that in such a disease as this we are bound to give our patients relief from pain if we can, no matter what the agent is, or how large the quantity or how long it may be continued. It has been said that a too early use of opium may lead to a habit which will injure the general health, but protracted pain, I believe, will injure the general health much more than any agent which may be necessary for its relief. If the milder agents will suffice for this, it is well, but there should be no hesitation, if they prove ineffectual, in at once resorting to the use of opium or any of its preparations which will best accomplish this. The special form of the agent which gives the most perfect immunity from pain with the least reactionary evil, should be carefully ascertained by trial. In some, neither opium nor any of its preparations can be tolerated, and in these the chloral has proved an inestimable blessing. A patient that I now have, who preferred to pass sleepless nights, tortured with pain, rather than endure the distressing nausea which followed a sufficient dose of any form of opium; and now she passes a blissful night of sleep by taking, on alternate nights, 30 grains of chloral. The effect is continued in a less degree the second night, but, if she takes it in any dose the second night, she suffers from both nausea and headache.

In some cases, there is great suffering from the extreme sensitiveness of the ulcerated surface, and this I have found best relieved by pessaries of iodoform, introduced at night into the vagina.

℞

Iodoform,	gr. x.
Butyri cacao,	ʒ i.
Glycerine,	gtts. 5.

M. One pessary.

These pessaries seem also to correct almost entirely the offensive odor of the discharges.*

As lotions for arresting the discharges and removing the fetid odor, I have found the most useful to be a solution of bromine, or of permanganate of potash, or of carbolic acid. Where the cervix or any portion of the vagina is ulcerated, I am careful to direct that these lotions should not be forcibly projected against the diseased surface by any form of syringe, but that the vagina be filled by a syphon-tube from a vessel placed considerably above the patient. The "fountain syringe" accomplishes this result in a very convenient manner.

* In the discussion of this paper before the Academy, Professor E. R. Peaslee stated that he had used an ointment of iodoform, ʒ i. to the ounce of lard, as an application to ulcerated epithelioma, with the effect of relieving pain, correcting the fetor, and notably diminishing the diseased mass.

APPENDIX.

ABSTRACT showing the amount of Mortality by Malignant and other Organic Diseases of the Uterus and Ovaries, in Three Periods, from 1804 to Oct. 1st, 1869—65¼ years—in the City of New York.

[From Fourth Annual Report of the Metropolitan Board of Health.]

PERIOD.	CANCER OF UTERUS.							UTERINE TUMOR.							UTERUS DISEASE (undefined).							OVARIAN TUMOR.							OVARIAN DROPSY.							TOTAL.
	Under 25.	25–30.	30–35.	35–40.	40–45.	45–50.	50 or upw.	Total.	Under 25.	25–30.	30–35.	35–40.	40–45.	45–50.	50 or upw.	Total.	Under 25.	25–30.	30–35.	35–40.	40–45.	45–50.	50 or upw.	Total.	Under 25.	25–30.	30–35.	35–40.	40–45.	45–50.	50 or upw.	Total.	Under 25.	25–30.	30–35.	35–40.	40–45.	45–50.	50 or upw.	Total.	
Ten years ending December 31st, 1865	9	18	9	7	13	9	168	431	3	..	5	..	2	..	17	27	4	1	5	..	2	..	8	20	6	5	8	..	1	1	13	43	3	3	7	10		5	28	549
1866, 1867, 1868, and first nine months of 1869	5	12	24	34	40	53	130	298	..	..	..	1	6	2	7	16	2	4	2	1	1	..	..	10	2	1	1	2	2	6	7	21	5	4	6	3	5	9	10	42	387

NOTE.—In the 52 years ending December 31st, 1855, the following statistics of death are recorded:

Cancer of Womb. None registered until 1837, when 6 deaths by that disease were reported, 7 in next year, 8 in next, and so on until 1855. The total number for the 52 years—but registered in only 15 years of that period—amounted to 247.

Disease of Ovaries and Dropsy of Ovaries counted together caused 44 deaths during the same period.

Unclassified Diseases of the Womb, 4 deaths.

Cancer (in what part of body not stated) caused 1,209 deaths.

Tumor (" " ") " 189 "

NOTE.—In the 10 years ending Dec. 31st, 1865, there was an average of 39 deaths yearly by "Cancer," region and kind *not stated*, in females. In the 3¾ years ending Oct. 1st, 1869, there was an average of 37 *such* deaths, but in the last 9 months, only 15 such deaths. But it will be noticed above that there was a yearly average of 79 deaths by Cancer of the Uterus during the last (3¾ years) period, and that the average in the previous ten years was only 43 per annum.

Again we see that counting fatal diseases of the womb and of the ovaries together in these two periods, we have a yearly average of 103 deaths in the latter period, and of fifty-five in the former or ten years period. This is an instructive view of the case, but we have reason to believe that at least 25 of the 103 (yearly) would have been classified as Uterine Hemorrhage, Asthenia, etc., had not our searching criticism of death certificates led to the correction of errors in such records.

The records of the past three years are probably as nearly correct in point of accurate and faithful diagnosis and certificates as could be wished.

E. HARRIS.

METROPOLITAN BOARD OF HEALTH,
OFFICE OF SANITARY SUPERINTENDENT,
No. 301 *Mott street, New York, Nov.* 27*th*, 1869.

MY DEAR DR. BARKER:—Your request to be furnished with the mortality statistics of malignant diseases of the uterus is cheerfully complied with; and were it only the bare numerical record which your object of inquiry demands, it would only be necessary to say that in the three and three-fourths years ending October 1st, 1869, there died of cancer of the uterus, in New York, 298 persons, and in the preceding ten years 431; and in the fifty-two years preceding these thirteen and three-fourths years there were registered only 247 deaths by cancer of the womb. Like other classes of statistics, these must be given very fully, and expressed in an analytical way and with precision, in order to tell the whole truth. It cannot be true that there has been such a tremendous increase in the number of fatal diseases of the womb which are registered as cancer as these three masses of the statistics would seem to indicate, even with liberal allowance for rapid increase of population. Permit me, therefore, to call attention to the fact that, since the Metropolitan Board of Health organized improved methods of inquiry and registry, we record no deaths from asthenia, debility, schock, hemorrhage unexplained, tumor, and other indefinite terms. Hence we find some increase in the accurately diagnosticated cancers of uterus, and a like increase in several other organic and dyscrasiac causes of death.

Besides this, we find that in the first fifty-two years of the sixty-five and three-fourths years, given in the accompanying abstract, there was a wide margin for indefiniteness in the statement of the causes of death. Yet I must confess that, after very careful analysis of the variable elements in the mortality records of the past sixty-five years, I am compelled to believe that malignant diseases of the uterus and ovaries are increasing far more rapidly than other dyscrasiac and organic causes of death in women.

I do not know your views on this question, but I fear that this sorest affliction of women—cancer of the uterus and ovarian diseases—will be found increasing even more rapidly than

the accursed practice of interference with the sacred function of the maternal organs.

I have endorsed a few facts upon the accompanying abstract of statistics to show how completely this kind of study of statistics on our death registers is beginning to demonstrate an increase of these diseases. Yet I wish to suggest that all comparisons of the present with the earlier statistics be entered in our arguments *cum grano salis*, for I well recollect that when we began our sifting and scanning process with medical certificates, we found deaths by cancer of the womb returned as *asthenia*, *debility*, and even as *consumption*, and occasionally as *tumor*, etc. But all this change, resulting from careful painstaking and a wonderful amount of good-will by the profession in revising certificates would not account for the rate of increase of deaths by cancer of the womb.

Very truly yours,
ELISHA HARRIS,
Sanitary Sup't.

To Prof. F. BARKER.

Extract from a Letter of Dr. Whitall, Resident Physician of the Colored Home, New York.

607 LEXINGTON AVENUE,
11*th Mo.* 10, 1867.

PROF. BARKER—DEAR DOCTOR:—In accordance with your request, I will state briefly my experience in the malignant diseases among the colored women. I have been connected with the Colored Home nearly five years, during which time about two thousand females have been under my immediate observation.

Malignant disease of the uterus I have seen but twice; in each case the woman was over fifty. Endometritis is quite common. Uterine fibroids are exceedingly common, the majority of middle-aged females having them.

My friend and predecessor, Dr. James D. Fitch, who had an

experience of twenty-five years among the colored, informed me that he had met with very few cases of malignant disease of the uterus.

Very respectfully yours,

SAMUEL WHITALL.

From Dr. J. C. Nott, formerly of Mobile, Alabama.

MY DEAR DOCTOR:—My surgical notes are somewhere nailed up among unpacked books which I brought from the South. I therefore can give you nothing very definite about cancer statistics in races. I have seen scirrhus in the breast and uterus in the full blacks, but much less frequently than in the whites. In the black male it has been most frequently in the medullary form. I cannot now call to mind a case of epithelioma in the black, and I am positive that it is very rare. Even with the whites, epithelioma is most common in the thin-skinned, with reddish hair. After our conversation on this topic I wrote to New Orleans, to Mobile, and to my friend Prof. Chisolm, late of Charleston, S. C., now of Baltimore. I send you his letter, which confirms my recollection; and if he had separated the mulattoes from the blacks the difference would have been much wider. The mulattoes are a feeble stock when crossed, *not on the Latin* races, but on the Anglo-Saxon or the Teutonic race. They are subject to scrofulous diseases, are short-lived, etc. With regard to the keloid facts stated by Chisolm, it is proverbial at the South that keloid is a rare disease among the whites, and the most common of all morbid growths in the negro; they will number, perhaps, as many as all others combined.

Very truly yours,

J. C. NOTT.

From Professor Chisolm, addressed to Dr. Nott.

BALTIMORE, *Dec.* 6, 1869.

MY DEAR DOCTOR:—I am in receipt of your favor, and will gladly put whatever information I have on this or any

other subject at your disposal. As regards the relative frequency of cancer in the various races, my experience sustains you, that cancer is not as common in the black race as in the white. I have seen all varieties in both, and I have just looked up some statistics which will sustain our views. In 1859, the State Registrar (of South Carolina) recorded and published the reports of 9,700 deaths: 2,423 among whites; 21 from cancer, 5 males, 16 females. Of 7,277 blacks who died, 29 deaths were from cancer, 5 males and 24 females. The population of the blacks were three times as numerous as the whites, and deaths from cancer only slightly increased. In 1865, in Charleston the mortuary record shows 2,068, of whom 560 were whites, and 1,508 blacks. Among the whites there were 3 deaths from cancer, among the blacks only 2. *En passant*, as the blacks and whites are nearly equal in population, this shows the relative mortality in the races. In 1866, the deaths in Charleston numbered 1,672: whites, 607, with 7 deaths from cancer; blacks, 1,164 deaths, 4 of cancer. In 1867 there were 1,341: of whites, 462, with 4 from cancer; blacks, 879 deaths, 5 of which were from cancer. In all of these cases the preponderance was greatly among females.

If the ever-recurring keloids be classified, as some do, among the cancers, then to this form the negroes appear peculiarly liable, their skins taking on keloid growth from any cause of irritation, as we have all seen from burns, blisters, incisions, even the cuts of the scarifier in cupping and boring the ears for ear-rings, etc. . . . . .

Your friend,

J. Julian Chisolm.

The following extracts are from a letter addressed by Dr. J. G. Gilmore, of Mobile, to Dr. Nott:—

"I am satisfied, however, so far as my limited experience enables me to form an opinion upon a subject to which my attention has never before been directed, that you are correct. I never saw anything like epithelioma in the black race. . . . If, in your large and long experience, you never saw it, it must be very rare. I have seen but one case of encephaloid in the negro since I have been in the city. . . . . To

make a rough estimate, I should guess that the relative frequency of scirrhus in the black and white females is about in the proportion of one to six. . . . Cancerous affections of the uterus are far more common in the white than in the black. . . . . I have seen, since you left us, more than a dozen cases of carcinoma uteri, and I recollect but one in the negro."

I may add that Dr. J. Marion Sims, from whom I had expected a note on this subject, and whose large experience in the South is well known, informed me that he had seen but two cases of malignant disease of the uterus in the negress.

A CONTRIBUTION

TO THE

SURGICAL THERAPEUTICS OF THE AIR-PASSAGES.

ILLUSTRATED BY TWO CASES.

By GURDON BUCK, M.D.,
SURGEON TO NEW YORK HOSPITAL, ST. LUKE'S HOSPITAL, ETC., ETC.

Case I.

Removal of a Morbid Growth from the Cavity of the Larynx by Laryngo-Tracheotomy, subsequent escape of a hard rubber trachea tube into the right bronchus, and its removal by operation.

John McGivney, æt. 38, native of Ireland, coachman, admitted into St. Luke's Hospital, April 15, 1870, with laryngeal trouble, of which the following is a description:—His voice is reduced to a hoarse whisper; his respiration is labored and obstructed alike in inspiration and expiration, and is accompanied with laryngeal resonance. Dyspnœa is aggravated by going up stairs, incautious swallowing, and other disturbing causes. Inspection of the fauces detects no other appearance than a general paleness of the surface, and digital exploration ascertains that the epiglottis is normal in size and position. Patient is thin in flesh and of sallow complexion, and has continued at his occupation up to the time of his admission into the hospital. He persistently denies any venereal antecedents, and has had no inflammatory attack affecting the air-passages. For years past his ordinary colds, he says, have been accompanied by vio-

lent coughing. About three or four months ago his voice began to be affected, and his breathing to be disturbed. Both troubles have gradually increased up to the present time.

Externally the region of the larynx presents no change in its appearance, nor does outward pressure have any particular effect. Patient's sensations refer to the larynx as the seat of his trouble.

The absence of all local inflammatory symptoms, together with the presence of symptoms of laryngeal obstruction, which have developed themselves gradually, point to the existence of a growth in the interior of the larynx as the cause of the trouble. This conclusion was confirmed by laryngoscopic examinations made by different experts. A growth (not papilliform) was seen in the cavity of the larynx below the vocal cords. Up to the 23d of April palliative treatment was used, but without affording any relief; on the contrary, the dyspnœa increased, and suffocating exacerbations became more severe and threatening. The danger of suffocation seemed so urgent that it was decided to resort to tracheotomy without further delay. Accordingly, a consultation having been summoned, the operation was performed at seven o'clock the same evening, and without etherization.

An incision was made in the median line from the thyroid notch downward to the sternal notch, and the tissues divided successively until the crico-thyroid membrane, the cricoid cartilage, and the three upper tracheal rings were laid bare. The struggling efforts of the patient during these proceedings aggravated very much the dyspnœa, so that in the extreme movements of respiration the cricoid cartilage would descend nearly to the level of the upper margin of the sternum. The crico-thyroid membrane itself, under pressure of the end of the finger, seemed very unyielding, as if there were resistance from within the laryngeal cavity. The operation having been unavoidably protracted, the dyspnœa had become very urgent, and suffocation seemed impending, when, without further delay, the trachea was opened by plunging a scalpel, with its back downward, in between the second and third tracheal ring, and cutting first upward through the cricoid cartilage, and then transversely along its upper margin, thus giving the opening a letter T shape.

Instantly the air rushed into the trachea with a whizzing sound, and on the insertion of a trachea tube the respiration soon became tranquil. The patient, who had long been deprived of rest, fell asleep before being removed to his ward. Nothing was attempted for the removal of the intra-laryngeal growth until the following day, April 24th, at 2 o'clock P.M., when a second operation was performed, with the aid of the other surgeons of the hospital and several invited professional friends.

Patient had passed a comfortable night, and was in good spirits and ready to be again operated on. It was now proposed to lay open the larynx in front, and expose its cavity for the purpose of removing the morbid growth from within. After etherization an incision was continued up to the os hyoides, and the thyroid cartilage exposed. A grooved director was passed into the cavity of the larynx, through the existing opening from below upward, and served to conduct the distal blade of a pair of scissors, curved edgewise, with which the thyroid cartilage was divided vertically through its notch, special care being taken to make the section exactly in the median line, and thereby avoid the vocal cords. The incision was also continued upward through the thyro-hyoid membrane. This permitted the two halves of the larynx to be drawn wide apart, exposing its cavity to full view. At first the entire cavity seemed to be filled up with the growth;—but a further careful examination ascertained that it occupied the right wall of the larynx, and involved the ventricle and both vocal cords underneath which it had originated, and which, together with the neighboring lining membrane, it had elevated and pushed inward so as considerably to encroach upon and narrow the laryngeal cavity. A small prolongation of the growth, somewhat pedunculated, emerged from the ventricle, between the cords, and protruded further into the cavity of the larynx. To remove the mass entire, an incision was made circumscribing its base and extending half an inch below the inferior vocal cord. It was carried down to the cartilage, from the surface of which the whole mass was detached from below upward. After dividing its connection above, it was found that the right arytenoid cartilage had been excised, and formed a part of the mass removed. No trouble-

some hemorrhage followed. Two or three points of suspicious appearance on the surface of the wound were touched with the actual cautery. Two thread sutures were inserted into the tissues covering the thyroid cartilage, to hold its cut edges in accurate coaptation, and the edges of the wound were brought together by two pin sutures above and one below the tube. In addition to these there were several thread sutures. A trachea tube of extra length was found necessary, owing to the depth of the trachea. Patient's subsequent progress was so favorable that in three or four days after the operation he left his bed and continued up and about. After the subsidence of the swelling he wore a hard rubber tube of medium size, corked at its outer end. His respiration was perfectly easy, and his voice remained very much the same as before the first operation. A copious viscid secretion was ejected through the tube by coughing.

May 10th.—The wound is entirely healed above and below the tube. The tumor removed was subjected to a microscopic examination, and found to be made up of small, round, nucleated cells, about the size of lymph cells, imbedded in a tolerably abundant fibrous stroma. The growth was of a pretty firm consistence.

Patient continued to do well up to Wednesday night, May 25th, when a very extraordinary accident occurred. Having gone to bed as well as usual, he was waked out of sleep about ten o'clock by violent coughing and struggling for breath. Soon, however, he became quiet and fell asleep, but was again waked up later in the night by a second attack similar to, but less violent than, the first. He now discovered that his trachea tube had disappeared, and, alarmed at the discovery, sent for the house-surgeon, who on examination found, to his great surprise, that the tube had disappeared, while the collar-piece (Fig. 1, a) which had supported the outer end of the tube, and to which the tape fastenings were attached, was undisturbed. The cork which had closed the outer orifice of the tube also remained *in situ*, occupying as it did a rim (Fig. 1, b) which formed a piece separate from the tube itself, but joined to it by a shallow-threaded screw. A separation had taken place at this joint, and the tube (Fig. 1, c) had dropped off, while the rim was still held in place by the

collar-piece. The house-surgeon passed a probe several inches down the trachea, and a probang down the œsophagus, but encountered nothing. Patient believing that the tube was lodged in the larynx, from a feeling of soreness in that part, the house-surgeon passed a finger of one hand into the larynx from above, through the mouth, and a finger of the other hand through the opening in the trachea below, and made them meet in the cavity of the larynx. He also had the patient elevated, with the feet upward and the head downward, thumping him at the same time between the scapulæ. At the regular visit on the following day, May 26th, patient continued perfectly free from any disturbing symptoms, and had no consciousness of any effect from the accident. Such a perfect state of quietude was thought to be incompatible with the presence of the tube in the air-passages, and it seemed a more probable conclusion that in the first paroxysm of coughing provoked by the presence of the intruding body, the tube had been projected into the fauces, and that the patient in his half-awake, half-asleep condition had swallowed it. Directions were accordingly given to watch his stools, in the hope of finding the tube.

The same uniform quiet condition persisted up to Wednesday, June 1st, when Dr. J. R. Leaming, one of the attending physicians, had his attention called to the case, and instituted a careful auscultatory examination of the patient's chest.

The doctor has kindly furnished me the following memorandum of his examination :—" In the left lung were signs denoting chronic pulmonary disease and the presence of viscid mucus, but no foreign body.

" In the right lung the breath sounds were peculiar in pitch and sonoriety, the respiratory murmur was everywhere distinct, though feeble, notwithstanding the peculiarity of the breath sound. This peculiarity had one point of greatest intensity, confined to the upper part of the interscapular space, about one inch and a half long vertically, and one inch wide, on the right side of the spine. Here the breath sound was heard as a modification of bronchial breathing, of equal length in inspiration and expiration, and of the same pitch in both. It was A in the musical scale.

" This was evidence of the immediate presence of the missing

tube, with the air passing through it. The lung immediately underneath the upper part of the interscapular space is supplied with air by the upper branch of the first division of the right bronchus, and the tube must have been engaged in this bronchus to have caused the maximum intensity of sound at this place. The length of the first division of the bronchus is not quite half an inch, consequently the upper end of the tube must have extended through the right bronchus into the trachea."

Having confidence in the accuracy of Dr. Leaming's examination, I directed notices to be issued for a consultation on the following day, Thursday, June 2d, at three o'clock. Dr. L. had already repeated his examination, and corroborated his conclusions of the day before. The patient, moreover, admitted that his breathing was not as satisfactory now as it had been, and auscultation showed that the respiratory sound had become less distinct over the right lung than over the left. My colleagues of the hospital and several other medical friends present examined the patient carefully, and corroborated the condition already ascertained. They also expressed their approbation of my proposal to attempt the removal of the tube by a surgical operation. It was performed as follows:—

Patient having been etherized, and the trachea tube removed, an incision was made from the lower margin of the existing opening, along the median line, to a point two inches below the sternal notch. The tissues subjacent to the skin were successively divided until the trachea was laid bare to the extent of one inch. To facilitate as much as possible the stretching apart of the edges of the wound, the incision of the skin and soft parts subjacent was continued a short distance upward above the opening. The exposed trachea was now divided from below upward, so as to communicate with, and very considerably enlarge, the existing opening. A needle threaded with a coarse ligature was passed through the edges of the wound, at opposite points, so as to include the cut edges of the trachea, and the ends of the ligatures tied to form loops with which to stretch wide apart the tracheal opening. While this was done by assistants, a flexible silver probe, seven inches long, fixed in a handle, and slightly curved in its whole

length, and bent into a small hook at its point, was passed down into the trachea, with its point directed to the right side. After passing to the depth of seven inches, the tube was encountered and its contact distinctly felt and appreciated by several of those present. Probably the end of the probe had passed through and beyond the tube, inasmuch as my colleague, Prof. H. B. Sands, having introduced his finger immediately afterward, announced that he could just reach it with the end of his forefinger. With the end of my little finger I now ascertained that the end of the tube presented itself obliquely to the axis of the trachea. I thereupon introduced a long and slender dressing forceps, bent edgewise at an obtuse angle (see Fig. 2), and with it seized and brought away the tube. The wound was closed as after the previous operation, and a trachea tube replaced. A copious expectoration of viscid mucus followed the operation, but without any bloody discoloration. The subsequent progress was favorable, and at the end of three or four days patient was again up and about the ward.

June 17th.—The wound has healed perfectly above and below the tube, and all inflammatory swelling of the surface has disappeared. Patient is quite free from cough and disturbance of respiration; his voice continues very much the same as before the first operation.

Desirous of returning to his home, patient was discharged wearing the tube.

On Monday, June 27th, he was subjected to a laryngoscopic examination by my friend Dr. F. Simrock, who has kindly furnished me the following report:—"Quite contrary to my expectation, I found the glottis quite patulous while the breathing is going on. The mobility of the left vocal cord is normal, and its excursion in approaching the right side of the larynx exceeds the median line. There is some ulceration yet to be seen at the former place of the right vocal cord, and it is impossible to see how far down toward the trachea the ulcerative process reaches. I would not have the least hesitation in leaving out the tube."

On Friday, July 1st, the trachea tube was left out entirely. Exuberant granulations, which had sprung up around the orifice of the opening through which the tube passed, were pared away

close to the skin, and the fresh-cut surface freely touched with nitrate of silver. A compress, spread with cerate, was applied over the opening, and secured in place by narrow strips of adhesive plaster.

July 7th.—Patient was exhibited at a meeting of the Academy of Medicine; the opening is reduced to a very small-sized chink.

26th.—The opening is entirely closed, a linear cicatrix in the median line of the neck marks the seat of the operation. Patient is directed to extend his neck to its utmost capacity several times daily for the purpose of lengthening the cicatrix.

Oct. 14th, '70.—Patient was examined by several medical gentlemen at St. Luke's Hospital. His general health is excellent. His voice has improved. Some words are now articulated in a clear tone, whereas previously his articulation was exclusively in a rough and hoarse whisper. Dec. 7th, '70.—Patient's voice has further improved. His articulation has become loud, though still hoarse, and is no longer whispering. Vowels and consonants are articulated with equal facility. He is unable to raise his voice above its ordinary pitch. His respiration is easy and natural, and he is free from cough or expectoration.

The tube extracted in this case measures separately two inches and five-eighths in length on its convex side, and two inches on its concave; its distal end is three-eighths of an inch in diameter, and its proximal end, where it joins the collar-plate, one-half inch outside measurement. Its convex side is perforated at its middle by a fenestrum one-half an inch in its longitudinal diameter. (See Fig. 1, c.)

By resorting to the cadaver it was found that the tube placed in the right bronchus could not be made to lodge entirely within the bronchus; its proximal end remained in the trachea at its bifurcation, and when placed with its convexity toward the right side the proximal end of the tube presented itself obliquely upward, which was the position it occupied at the time of the operation, as recognized by Prof. Sands as well as myself. When, however, the position of the tube was changed so that its convexity looked to the opposite side, toward the median line, the proximal end then looked directly upward toward the larynx, and its convex side near the extremity was no longer

within reach of the finger, as it was in the first position. This is shown in Fig. 3.

The state of complete quietude following the first disturbing effects of this extraordinary accident, and persisting as it did for one week afterward, may be explained by the fact that the tubular form of the foreign body, largely perforated in its middle, caused very little or no obstruction to the passage of air. Its form, too, being curved, both ends of the tube rested against the walls of the passage in which it was lodged, and tended to hold it at rest. This condition of quietude, however, had reached its limit. The patient on the day of the operation, just one week after the accident, began to experience some difficulty in respiration, and the respiratory murmur had become more indistinct over the right lung. A longer delay of the operation would no doubt have developed increasing manifestations of obstruction.

THE OPERATION.

The introduction of forceps and other instruments into the trachea and bronchi, through an opening in the neck, for the removal of foreign bodies has been successfully practised in numerous instances recorded by different authors. The introduction of the finger, however, into the trachea, for the purpose of ascertaining the situation of a foreign body, is a proceeding altogether new within the knowledge of the author. The facility of this operation in the present case, and the practical advantage of it in guiding to the prompt and easy removal of the foreign body, furnished a very suggestive practical lesson to all present on the occasion.

My worthy colleague, Prof. H. B. Sands, to whom belongs the priority in digital exploration of the interior of the trachea, joins me in not only sanctioning but recommending the practice as safe, and as affording a sure guide to further proceedings for the removal of the body we are in search of. Its application is so momentary as scarcely to occasion any impediment to respiration. It may be directed not only downward toward the bifurcation, as in the present case, but also upwards through the larynx, to ascertain the condition of the vocal cords and ventricles, and also of the epiglottis and its surroundings. It

would be of special advantage where a foreign body, as has often happened, has become lodged in one of the ventricles.

The operation performed in this case proved to be well adapted to the employment of digital exploration. The opening in the trachea must necessarily be very free, and a vertical incision should be made of at least one inch in length, commencing at the lower edge of the cricoid cartilage, and dividing the five upper rings of the trachea. To make this opening capable of the greatest degree of dilatation, a transverse incision should also be carried across its upper extremity, between the first tracheal ring and the cricoid cartilage, to the extent of half an inch on either side. (See Fig. 3.) A ligature passed through the opposite edges of the wound, and including the tissues immediately in contact with the trachea, but without perforating it, with the ends tied together so as to form a loop, affords the most convenient means of stretching the edges of the opening apart. The division of the isthmus of the thyroid body, necessarily involved in this operation, need not be dreaded. The author, in his numerous operations for tracheotomy, has never had any special difficulty from hemorrhage from this part, nor has he ever taken the precaution to previously ligate the isthmus on either side of the median line and then cut between the ligatures. Such a precaution might easily be taken, and would be a complete security against hemorrhage.

After previous digital exploration, the forceps best adapted to seize a foreign body in the trachea below, or in the larynx above, is the one represented by Fig. 2.

Should the foreign body be lodged in either bronchus, it might still be reached, and though not seized, it might be dislodged and expelled by coughing.

Remarks.—We have in the case just narrated a rare and exceptional instance of a foreign body getting into the air-passages without passing through the larynx. The few examples on record of this mode of entrance have been most frequently those occurring in gunshot wounds. A memorable exception is, however, reported by De la Martinière, in vol. v. of Mém. de l'Acad. Royale de Chirurgie, p. 346, of a copper pin fifteen lines in length that pierced the trachea just below the cricoid

cartilage, transfixing the tube and penetrating its opposite wall. Having no head, the pin buried itself below the surface of the skin at its entrance.

In Professor G. D. Gross's very thorough and able Treatise on Foreign Bodies in the Air-Passages, he enumerates a great variety of substances which have got into the air-passages. Among those of a vegetable nature were beans, grains of corn, seeds of different kinds, pits of fruits, nut-shells, ears of grass and rye, cockleburs, etc. Among those of an animal nature were bits of meat, bone, shell, claws, quill, teeth, human and animal, besides a variety of other substances. Buttons, pins, needles, beads, shot, coin, nails, and other metallic substances were also on the list.

M. Bourdillat, in an article, "Observations pour servir à l'Histoire des Corps Étrangers dans les Voies Aériennes," in *Gazette Médicale de Paris*, No. 7, 15 Fevrier, 1868, has collected three hundred cases of foreign bodies in the air-passages, of which seventy-eight were cases of beans, sixteen of cherry pits, nineteen of prune pits, eighteen of pieces of bone, five of coin, nine of teeth, six of water-melon seeds, besides a great variety of other substances. Neither author furnishes any instance of a trachea tube dropping into the air-passages, as in the case now reported. In his researches, however, the author has discovered one other case, recorded in the *Brit. Med. Journal*, Feb. 15, 1868, p. 141, by J. Waters, M.B., Reigate, England, which is given below entire.

T. W., aged 40, tailor, twelve years ago had tracheotomy performed for disease of the larynx and glottis following fever. He had worn the tube ever since, not being able to do without it. The voice had been distinct, but husky. On March 22, 1867, at 8.30 A.M., while he was attempting to remove the tube, as he was accustomed to do for cleansing, the rim came off and he could not get hold of the cylinder. When I saw him shortly afterwards I could see the tube at the bottom of the wound. I went down stairs to get instruments from the carriage, and to ask Dr. Holman to assist me. When I returned the tube had slipped down the trachea, and could only be felt by a bent probe at a distance of five inches from the external opening, to the right. Respiration was imperfect over the right side of the chest, perfect on the left. Attacks of dyspnœa were at times moderately severe, and cough very troublesome. Inversion was tried, with smart blows on the front and back, without altering the position of the tube. At 3 P.M., having obtained a pair of long-curved forceps, we resolved to make further attempts to remove the tube. A stilette, bent at

the end, was first introduced, and caught something which seemed like the tube, but on making traction, smart bright hemorrhage came rather freely from the tracheal wound, so the hook was disengaged and withdrawn. Chloroform was then administered over the mouth and trachea. It excited him much at first, but he soon went under its influence; the long forceps were then introduced and the tube was felt, but it could not be caught; bloody mucus came freely from the mouth and wound, and he retched freely. The respiration became so much impaired that the chloroform had to be given up, and time given him to eject the accumulation in his air-passages. This condition being somewhat perilous, when he had sufficiently recovered I made a fresh examination, and found the tube rather more than five inches down, and to the left. After one or two failures, the tube was seized with the long forceps and drawn to the wound, where it stuck, and while Dr. Holman was enlarging the opening to admit its removal it slipped back. I managed, however, to catch it again immediately, fortunately, this time, in the long axis, and it was readily removed, and found to be filled with mucous membrane. After the removal he complained merely of being weary and of some soreness of the chest. He suffered, during the attempts at removal, merely distress from obstruction to the breathing. His recovery was very rapid, and in a few days he was able to resume his work.

An important point worthy of notice in this case is the condition of the voice after the removal of the vocal cord and arytenoid eminence of the right side. In this respect the case is believed to be quite unique, and without example in the records of surgery.

The following report of the condition of the interior of the larynx, as ascertained by a laryngoscopic examination made by R. F. Weir, M.D., has been furnished by him :—

"The left vocal cord is drawn beyond the median line toward the opposite side in phonation. The cicatrix presents a sharp, curved line, running backward, and appearing to be attached to the opposite arytenoid cartilage, and lost in the adjacent pharyngeal tissues. This bridle-like cicatrix moves slightly to meet the remaining vocal cord in phonation. Below is a reddish projection, and still farther below, and anterior, is a yellowish mass that I take to be the cicatrix of the trachea.

"R. F. Weir.

"Dec. 13, 1870."

Case II.

A fish-bone lodged in the larynx during a period of fourteen years.—A trachea tube inserted after laryngo-tracheotomy, and worn twelve years and then dispensed with.

On the 22d May, 1855, Mrs. F. C. J., æt. 25, daughter of a physician residing at N. R., while eating small fish at dinner had a bone lodged in her throat, which at once caused great disturbance and violent efforts of coughing and hawking to dislodge the foreign body. Her father improvised a probang and passed it down the œsophagus, and resorted to various other measures for her relief, but without success. She could reach the bone on the left side of the throat with her left forefinger, and perceived that the end of the bone pointed upward. The same evening, at about 9 o'clock, while taking some food, the bone became dislodged, and complete relief followed, to her great joy and that of her family. For eight days she continued well, with the exception of slight remaining soreness, and then began to cough. As she had been somewhat exposed in a visit to the city, the cough was attributed to her having taken cold. Soon the cough became croupy and sonorous, and very frequent, especially at night. Her husband was struck with the croupy character of the cough from the outset. Soon the voice began to be affected with hoarseness, at first only at night when suffering from attacks of dyspnœa; subsequently, however, the hoarseness became constant. Suffocative attacks at night increased in severity, and sometimes were very alarming. The use of opiates at bedtime, such as Dover's Powder, or M'Munn's Elixir of Opium, became indispensable, and would often keep her easy for the night. She writes of her own case as follows:—"On the night of 7th July, 1855, I was seized with severe coughing, accompanied by a great sense of oppression, together with much distress and aching about the throat. Warm fomentations and inhalations were tried, but without any decided relief. The distress continued for seven hours, gradually diminishing as daylight appeared. Since that time I have been constantly subject to similar attacks, varying greatly in frequency and degree. Sometimes they would return

as often as once a week, usually taking about two days to reach their height and pass away. At other times several weeks have elapsed without more than a slight return of the difficulty. During the most severe paroxysms the sufferings were intense, the breathing was hard and labored, accompanied by a sense of suffocation, which was greatly aggravated by the least excitement, or even by any motion of the body such as walking across the room, &c. The effort of going up stairs would be followed by very great oppression, so as to be at times almost beyond endurance. The distress generally increased at the approach of night; the darkness seemed to aggravate the difficulty. It was also necessary often to maintain an erect posture in bed, with the head inclined forwards, being the only way to obtain relief in breathing. The last attack of the kind was on the 8th January, 1858. After a day of severe and almost incessant coughing, the difficulty of breathing became very manifest as night drew near, and when aggravated by coughing it almost amounted to suffocation. I could not endure the darkness, and was obliged to keep the gas burning through the night. The cough was kept partially under control by means of an anodyne, without which it seemed to me I could not live till morning. The distress diminished as daylight appeared, and the next day found me more comfortable." There was nothing visible or tangible in the throat. Deglutition was easy. Sometimes she felt pain darting through the throat, and also soreness in a particular spot on the left side when pressure was made with the finger over the thyroid cartilage. She has had no fever at any time, has felt in other respects well, and had never previously been subject to cough or throat trouble. The cough has been without expectoration from the first. July 7th, 1855, nearly seven weeks after the accident, I saw Mrs. J. for the first time, in consultation with her father, her uncle, and Dr. Voris, her attending physician. The conclusion arrived at, as the result of our consultation, was, that Mrs. J.'s ailment was unconnected with the accident of the fish-bone. An application of leeches over the larynx, a mild mercurial course, alum emetics, and inhalations of vapor were advised and tried, but were followed by only temporary benefit. The mercurial was afterwards resumed, and blisters applied over the larynx. Anodynes were also given

at bedtime. No permanent benefit followed; on the contrary, her condition grew steadily worse. Dyspnœa became constant by day as well as by night, and was aggravated by every exertion. Mrs. J. having come to the city, a second consultation was held on the 29th September, 1855, in which Drs. Cammann, Willard Parker, and John Watson joined the original consultants. Each gentleman visited and examined the patient separately the day before. After a full discussion it was unanimously agreed that the origin and persistence of the symptoms depended on the presence of the fish-bone in or near the larynx, and that a surgical operation was the only resource from which relief was to be expected.

From this time onward Mrs. J. continued to suffer from cough and suffocative attacks with varying degrees of severity. At no time was she free from hoarseness, though in her best moments it was slight. Breathing, even when most tranquil, was accompanied with an audible laryngeal resonance. She continued to be without expectoration, except on the supervention of an ordinary cold, when the sputa presented the ordinary appearance of mucus.

Her suffocative attacks recurred frequently at night, and often with an alarming severity. Her general health suffered no material change till the end of the summer of 1857, when, on the 22d August, I was again requested to visit her at her father's residence in the country, in company with her uncle, Prof. J. M. S. The night previous she had had an attack of unusual severity, which had so much alarmed her father that he had sent to the city for assistance. We found her relieved of her urgent symptoms, but still suffering from the exhausting effects of the last night's attack. From this time till early in the following month of January (1858), when she came to the city to reside, Mrs. J. felt that her general health was steadily deteriorating; her suffocative attacks had become more frequent and severe, and during the intervals her relief was only partial. It should be remarked that during the whole progress of her ailment Mrs. J. has experienced a circumscribed soreness, referred to the left side of the larynx, over the thyroid cartilage. The failure of her general health, and the aggravation of her symptoms experienced during the past three or four months, have

brought Mrs. J. to feel not only willing to submit to an operation, but desirous of it. Her husband shares the same feeling, and her parents also are convinced that it is the only resource from which relief can be expected.

On the 7th January, 1858, another consultation was held, in which Dr. Cammann, Professors A. Clark and Joseph M. Smith, as physicians, and Professor W. Parker and Dr. J. Watson, as surgeons, took part with the writer. Each consultant visited and examined the patient separately before the consultation.

After a free expression of opinion and discussion, the following conclusions were arrived at:—

1. That there was no evidence of tuberculosis or other disease in the lungs or elsewhere.

2. That Mrs. J.'s condition is to be attributed to the continued presence of the fish-bone swallowed in the month of May, 1855, all her symptoms dating their origin from that occurrence; and inasmuch as a sore spot has uniformly been felt, and referred to the left side of the thyroid cartilage, and to a point corresponding to the situation of the left ventricle of the larynx, it is thought possible that the bone is imbedded in the ventricle.

3. That inasmuch as no relief had been afforded by any medical treatment hitherto employed, but, on the contrary, the patient is steadily growing worse, and is in constant danger of suffocation from the increasing laryngismus, a surgical operation is required, and should be resorted to without delay.

4. That in order to expose the interior of the larynx, and to remove the foreign body supposed to be lodged in it, and also to provide, if necessary, for the establishment of a tube permanently in the trachea, with the view of thereby leaving the larynx itself at rest, the operation of laryngo-tracheotomy is to be preferred in this case. On the 10th January, at 10 o'clock, another consultation was held, when the same consultants were present, with Professor A. H. Stevens added to their number. The former conclusion was confirmed, and two o'clock of the same day was appointed for the performance of the operation.

OPERATION.

Patient was placed on a narrow bed facing a window, and the bedstead elevated at its head so as to form an inclined plane. Ether having been administered, an incision was made in the median line, from the os hyoides to the third tracheal ring, and the tissues successively divided till the cartilages were laid bare. The hemorrhage, which was moderate, was allowed to cease before incising the larynx. The crico-thyroid membrane was first pierced in the median line, and the incision continued upward from the upper edge of the cricoid cartilage through the lower half of the thyroid cartilage. At this moment the air rushing in through the opening, the lungs seemed as though taken by surprise, and for an instant intermitted their action. In order to afford prompt and free access of air to the lungs, I immediately extended the incision downward through the cricoid cartilage and the two upper tracheal rings, and stretched the edges of the opening apart. As soon as the respiration had become quietly established through the wound, I divided the upper half of the thyroid cartilage very exactly in the median line through the notch. The next step was to make a thorough inspection of the interior of the larynx, especially of the left ventricle. The attempts to do this, by stretching apart the edges of the wound, excited a good deal of disturbance, such as coughing, suffocation, etc., so that we were compelled frequently to desist and allow patient to recover herself. After an hour and a half occupied in these proceedings, patient became so much exhausted that it was thought most prudent to postpone further examination to the following day. Respiration being performed with entire facility through the wound, a tube was not inserted. The examination, so far as we had been able to make it, ascertained the existence of a spot, of the size of a small split pea, on the inner surface of the cricoid cartilage, posteriorly and to the left side, where the mucous membrane was wanting and the surface was of a deep red or purplish color. The cartilage was not exposed, nor could a probe detect any track leading from it. The ventricles appeared to be in a normal condition, and the finger passed upward through the glottis could perceive

nothing abnormal. The wound was left open, and kept covered with compresses wet in tepid water. The pulse, which had risen to 120, subsided to 100. The succeeding twenty-four hours were passed comfortably. A viscid mucous expectoration, stained with blood, was expelled through the wound by coughing. On the following day, at 12 o'clock, the examination was resumed in the presence of the same gentlemen as attended the day before. The parts had now become very sore from inflammation, which rendered the examination very painful, especially as ether could not be administered. The divided edges of the larynx were stretched apart, and the interior surface carefully inspected. The spot already noticed was seen, and its appearance verified as described above. No pus was observed on its surface or in its neighborhood. A thin layer of lymph exudation, the product of recent inflammation, coated the posterior surface of the laryngeal cavity. In order to establish a tube in the trachea, it was necessary to expose and divide two additional trachea rings at the lower angle of the wound. After doing this the upper ring was detached from the lower edge of the cricoid cartilage to the extent of two lines on either side of the vertical slit, and a triangular piece clipped away with scissors from each angle, including portions of the three upper rings. This converted the vertical slit in the trachea into a triangular opening, with its base upward. A silver trachea tube was then inserted, and secured by tapes passed around the neck. The wound above the tube was closed in the following manner:—

1st. By a single suture, inserted into the muscular and aponeurotic tissues covering the thyroid cartilage on either side of the median incision. This held the opposite edges of the cartilage in exact coaptation. It was observed before introducing this suture that in every act of deglutition the cut edges of the thyroid cartilage were drawn apart by the action of the constrictor muscles of the pharynx.

2d. Four interrupted thread sutures were inserted into the edges of the integument, between the tube and upper angle of the wound.

3d. The whole was supported by strips of adhesive plaster crossing from one side of the neck to the other, between the sutures. Before closing the larynx, the ulcerated spot no-

ticed on its inner surface was freely touched with nitrate of silver.

The constitutional disturbance following the operation was at no time very considerable. Within one week the pulse subsided to below 80. The cough was variable, sometimes severe and in paroxysms, and at other times suspended for long intervals. The expectoration ceased to be bloody after the first twenty-four hours, and during the first five or six days was opaque, viscid, and rather copious, and then gradually became transparent, frothy, and much less in quantity. During the first week the tube required to be changed twice in twenty-four hours; after that, but once a day. There was no appearance of pus at any time in the expectoration, nor was there any feeling of soreness around the track of the tube. The respiration through the tube was from the first perfectly free. The wound healed above the tube by first intention, and at the end of three and a half weeks it had also healed around the lower side. Three weeks after the operation a tube, perforated at its middle on the convex side, was worn, and with it the patient enjoyed the use of her voice by corking the outer orifice of the tube. At first articulation was performed in a hoarse whisper, and respiration through the larynx was attended with some effort; but gradually the obstacles diminished, and at date of Feb. 13, 1858, Mrs. J. wears the tube for hours, and is not sensible of any difference in breathing with or without the cork inserted. Her general health is greatly improved.

Under date of July 13, 1869, Mrs. J., at my request, furnished me the following statement:—"In regard to the condition of my health while wearing the tube, I can say there has been a manifest improvement. The principal difficulty has been an inability to take any violent exercise or motion. The effort of going up-stairs, or making any ascent, is followed by more or less difficulty of breathing. The cough has never left me, though, unless aggravated by cold, it does not cause much inconvenience. There have been occasional returns of the suffocative attacks, though at intervals of several months, similar to the former ones, but varying in degree. Wearing the tube has caused very little discomfort; on the contrary, I am hardly conscious of its presence. There has been no constant soreness

in the region of the tube, though occasionally the outer surface around the opening through which it passes into the windpipe has become swollen and inflamed, causing a good deal of inconvenience. I have always kept two tubes, one for the day, the other for the night, and have changed and cleaned them regularly morning and evening." During the long period of twelve years that Mrs. J. was obliged to wear the trachea tube she abated nothing of her accustomed activity in the discharge of her ordinary duties. In the summer season she often accompanied her friends on distant travelling excursions. The tube being concealed from view by the arrangement of her neck-dress, there was nothing to betray her condition to casual observers. A slight huskiness of voice, and that by no means conspicuous, was all that was noticeable.

One morning in the month of June, 1869, Mrs. J. having, as usual, removed the tube to cleanse it, noticed something sticking to its outer surface, on the left side, near the fenestrum, to which she called her father's attention, and which, on minute examination, proved to be a small fish-bone, about five-eighths of an inch in length, curved, flattened, and triangular at one end, but growing smaller and tapering at the other end.

For several weeks previous to this occurrence she had suffered from a frequent accumulation around the inner end of the tube and the opposite surface of the trachea, of viscid, tenacious secretion, that would obstruct breathing and provoke violent coughing paroxysms for its expulsion. Often she was obliged to remove these accumulations by seizing the matter with a forceps inserted into the opening, after first removing the tube. Sometimes they came away in a tubular shape, and were of very tough consistency. Complete relief always followed their removal.

The important question of dispensing wlth the trachea tube now urged itself upon our consideration. Mrs. J. and her family naturally felt great hesitation in making the experiment, fearing as they did that the closure of the opening might narrow the calibre of the trachea to such a degree as to obstruct respiration. This fear was met by the consideration that the tube itself occupied more space in the trachea than would be lost by the contraction consequent upon the closure of the open-

ing, if any took place. The author could also cite from his own observation a case in which tracheotomy having been performed, and a triangular portion of the two upper tracheal rings having been excised, the tube, after being worn for several months, had worked its way gradually up into the larynx. At a post-mortem examination, no trace of loss of substance could be detected; the rings of the trachea were identified and found complete from the cricoid cartilage downward. The specimen is preserved in the Pathological Cabinet of the New York Hospital. The reproduction of the tracheal rings seemed thus to be conclusively established. The question of removal of the tube, however, continued in abeyance till January 21, 1870, when Mrs. J., having come to the city for the winter, was disposed to undertake the experiment. Before doing so, however, it was thought best to ascertain the condition of the interior of the larynx by a thorough examination with the laryngoscope. This was done very satisfactorily by my friend Dr. F. Simrock, not only by the application of the mirror above through the fauces, but also below through the opening in the neck. It was ascertained that there was no thickening of the vocal cords or other parts, nor any contraction of the laryngeal cavity. The inner surface of the trachea, seen through the opening in the neck, under exposure to sunlight, presented patches of adherent viscid exudation, such as, for some time previously, had accumulated and occasioned obstruction of the respiration. To expel these accumulated secretions violent paroxysms of coughing were provoked, as already noticed. Encouraged still further by the result of the laryngoscopic examination, Mrs. J. now consented to the removal of the tube, which was left out. The opening situated in the median line of the neck, immediately below the cricoid cartilage, is about two-thirds of an inch in its vertical diameter, and five-eighths of an inch in its tranverse. Its margin is even and flush with the neighboring surface, which is somewhat sunken below the level of the surrounding parts. The skin is supple and movable up to the border of the opening. To facilitate the dislodgment and expulsion of the viscid tracheal secretion, patient was directed to inhale frequently the fumes from an infusion of catnip, and her father was to make an application to the trachea, through the opening, of a solution of

nitrate of silver, of the strength of ten grains to the ounce, morning and evening. A compress, secured in place by strips of adhesive plaster, was worn over the opening to prevent the passage of air in respiration and coughing. Patient continued to be disturbed by the accumulation of viscid secretion in the trachea, and the cough provoked by it, till 21st February, when the external opening, after gradually diminishing in size, had become reduced to less than one-fourth its original dimensions, and the tracheal secretion had begun to make its way out through the larynx and mouth instead of through the tracheal opening.

May 28th.—Thirteen weeks having elapsed since the opening ceased to diminish in size, it was decided to perform an operation for its permanent closure.

OPERATION.

Ether having been administered, the margin of the orifice was pared away, and an incision extended a short distance above and below the opening. The edges of the skin were then dissected up on either side so as to permit broad raw surfaces to be confronted and secured in contact by means of three pin sutures, wound with cotton yarn, and intermediate thread sutures. Patient was directed, whenever she coughed, to make pressure with her fingers over the wound, so as to prevent the skin from being pushed away from the subjacent parts by the air seeking to force itself out through the tracheal opening.

June 2d.—Primary union has taken place except at two points, which permit an escape of air and frothy pus in small quantity. To close these, fresh sutures were inserted.

June 18th.—The closure of the wound is now complete, and the skin adherent to the subjacent parts. The opening in the trachea is not only closed by the skin covering it, but a gradual growth of the tracheal rings across the opening has been going on since the tube was left out. This process has been appreciable to the touch. A vertical slit, less than one-fourth of the size of the original opening, is all that now remains. Patient will now leave for the country, and will in a few days leave off the use of an elastic rubber band and compress which she has worn to support the parts.

June 30th.—Mrs. J. called to permit a final inspection. Everything about the wound is doing well. A vertical linear depression on the surface of the trachea is all that can be felt through the superjacent skin to mark the situation of the tracheal opening. The reproduction of the excised portion of tracheal rings appears to be complete. The parts involved in the operation are sunken in a hollow below the level of the surrounding surface. The tracheal secretion has lost its tough viscid character, and is much diminished in quantity. Respiration has suffered no impediment since the closure of the trachea.

Dec., 1870.—Mrs. J. continues to do well.

The question of chief interest suggested by this case relates to the whereabouts of the fish-bone during these long fourteen years of its sojourn in the larynx. The most satisfactory conclusion at which the author has been able to arrive in the retrospect of the history of the case is, that the bone, after producing immediate violent disturbance, was suddenly dislodged while Mrs. J. was taking food, and located itself in a situation where, for a few days, it ceased to provoke any disturbance. At length, however, it made its way into the left ventricle, where it lay concealed, and escaped detection when the cavity of the larnyx was laid open and explored. This conclusion is, perhaps, strengthened by the fact that a local soreness was felt by the patient during the whole course of her suffering on the left side of the larynx, and was referred by her to a spot corresponding to the situation of the left ventricle, at which spot outside digital pressure also produced pain. It will also be remembered that the fish-bone, when first discovered, was adhering to the surface of the trachea tube on its left side and near its middle. It is difficult to conceive of a substance like the fish-bone imbedding itself in the soft parts lining the cartilaginous parietes of the laryngeal cavity without ulceration and suppuration. The membrane lining the pouch-shaped ventricles, however, being lax and more or less wrinkled, the fish-bone, so minute as it was, might easily be concealed in its folds and escape detection. That it should have remained for so long a time is certainly very remarkable.

Fig. 4 shows the trachea tube worn by patient.

Fig. 5 the size and shape of the fish-bone discharged.

ON DIABETES.

Read before the New York Academy of Medicine, February 2, 1871.

By GOUVERNEUR M. SMITH, M.D.,

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Mr. President and Fellows of the New York Academy of Medicine:—The disease known as diabetes, though recognized as a distinct disorder for many years, and made a subject of philosophical medical research, has still lingering about it many unsolved questions relating to its etiology, pathology, and treatment. It is, therefore, a malady almost as attractive for our study as though it was a new field of inquiry. Aided by the facts which have been accumulated by predecessors and cotemporaries, we are justified in anticipating an early solution of several of the vexed questions to which I have alluded.

The physiologists have partially explained the phenomena of glycogenesis; the pathologists have not as yet been equally successful in describing the conditions favoring glucosuria. As clinical observers are to-day better armed and equipped to investigate the subject, it is but becoming that we should at once proceed to systematically arrange relevant physiological and pathological facts, and, binding them together with an Ariadnean thread, present an array of truths useful in their practical bearings.

In opening the discussion in this Academy this evening, I must express regret that I have not been more successful in unravelling the knotty questions surrounding the subject, but must hope that the statements and inferences presented will not be

wholly devoid of interest, and may at least evoke a fuller elucidation of the entire subject at your hands than it is in my power at present to offer.

While the term Diabetes was employed by the older medical writers, it did not convey the same significance of meaning that it does at the present day. Derived from the Greek, δια, through, and βαινω, to pass, it simply implied an immoderate flow of urine. A new importance was attached to the expression when Dr. Thomas Willis, of Oxford, about two centuries ago, declared that the urine in diabetes contained saccharine matter. In 1659 this gentleman published a treatise on fermentation, another on fevers, and another on the urine. He subsequently removed to London, where, from the distinction he had earned from these elegant Latin dissertations and other works, he was appointed physician to King Charles II., and doubtless was the medical adviser of his monarch during the great plague in 1665.

The advance made by Dr. Willis in his investigation of the subject of diabetes was, however, quite insignificant, and his views in regard to the presence of sugar in the renal secretion were merely presumptive. More than a century elapsed before further light was cast upon it, when Dr. Matthew Dobson, of Liverpool, in 1779, proved the truth of Dr. Willis' hypothesis by extracting sugar from the urine voided by a patient suffering with the disease under consideration.

As excessive diuresis is a prominent symptom in diabetes, it is not surprising that our earlier predecessors attributed the malady to some nephritic lesion; indeed, comparatively little attention was directed beyond the kidneys until the year 1835, when Ambrosiani, of Milan, by detecting glucæmia, showed that the distemper might be either of humoral origin or be the result of lesions in viscera remote from the renal emunctories. It has consequently only been within about forty years that pathologists have been very carefully searching for the causes of glucosuria beyond the kidneys; and the reason for their previous remissness in this regard must be partially attributed to their limited knowledge of chemistry and physiology.

Such oversight, however, was scarcely pardonable, when we recall the fact that the illustrious Boerhaave, in the early part

of the last century, from his chair in the University of Leyden, called attention to the fact that it is hardly possible to decipher the precise nature of disease from a mere inspection and examination of the urine. Such examinations were as common in his day as in our own, but necessarily lacked the precision of modern science.

After showing how the urine may be altered in quantity, in color, and in other qualities by the use of diuretics and other drugs, Boerhaave remarked: "By these artifices I saw two professors of this University imposed upon, who, having well considered the urine, without being acquainted with what the patient had taken, pronounced their judgment according to the rules of art." Are we not liable, at the present time, to fall into somewhat similar errors? It has been well established by recent research, that the renal secretion can be materially modified in its character by the use of diuretics. Some drugs may simply induce a hydragogue effect upon the kidneys; others may facilitate the excretion of organic principles; others again of inorganic substances: while some medicines will be themselves excreted by the emunctories alluded to, a short time after their ingestion. In our ordinary examinations of the urine, are we not prone to overlook these considerations; and do we not occasionally draw imperfect conclusions from our testings?

Boerhaave, in enforcing the importance of studying other signs of disease, further says: "And above all, the physician must avoid presaging anything more from it than what relates to the disease. For certain it is that no mortal can tell by the urine whether it came from a man or a woman, except, perhaps, some of the animalcules washed off by the urine after a seminal flux, should by the help of a microscope discover it to belong to a male; much less can any one determine whether the person to whom the urine belonged was a virgin, a married woman, or with child; all which pretensions deserve only to be laughed at. I remember two professors many years ago, in this city of Leyden, who were egregiously deceived in their uromancy, when, according to the rules of their art, they denied a certain noble lady was with child till the birth of the infant silenced the prophets. I have known the principal urine judges in this Republic to have been guilty of such egregious and glaring

errors, that if they had the least shame or merit they would never have appeared in their character."

These views of Boerhaave are thus extendedly presented, in order to show that while he did not underrate the value of an examination of the urine in disease, he did not place undue reliance upon it, and that he called the attention of the medical men of his generation to the importance of examining other morbid signs when the renal secretion was abnormal in character.

As, however, excessive diuresis occurred in diabetes, the idea became prevalent that the disorder was of nephritic origin; and the error, becoming established, has been for the most part perpetuated in nosologies, and glycosuria treated of in works on urinary diseases to the present day. One of these points may be illustrated by exhibiting the status of the malady in several of the more important classifications of disease which have been employed for the last century.

Diabetes Mellitus.

Nosology.	Date.	Class.	Order.	Genus.
Sauvages	1762	Fluxus	Serifluxus	
Linnæus	1763	Evacuatorii	Genitalium	
Vogel	1772	Profluvia	Apocenoses	
Sagar	1776	Fluxus	Serifluxus	
Cullen	1785	Neuroses	Spasmi	
Darwin	1796	Diseases of Irritation	Retrograde Irritative Motions	Of the Absorbent System
Crichton	1804	Fluxus	Fluxus cum Febre	Diabetes
Pinel	1809	Læsiones organicæ	Læsiones organicæ particulares	Sectio Læsiones organorum uropoieticorum — Order Diabetes
Parr	1809	.....	Profluvia	Apocenosis
Swediaur	1812	Cachexiæ et cacochym	Dyschymiæ	
Young	1813	Pareccrises	Cacochymiæ	
Good	1817	Eccritica	Paruria	
Hosack	1818	Profluvia	Apocenoses	Diabetes
Metropol. Board of Health, New York	1866	Local Diseases	Urinary System	
Royal College of Physicians of London	1869	General Diseases	Diabetes	

In viewing this table, it is noteworthy how little can be gleaned concerning the pathology of diabetes. Cullen placed the disorder in question in the class Neuroses, and in the order Spasmi. Hosack, in criticising the nosologists who preceded him, remarked in regard to Cullen's classification of diabetes: "I have excluded many of those retained in that class by Cullen, as *cholera*, *diarrhœa*, and *diabetes*, which more properly belong to the class profluvia, or excessive evacuations;" and again he continued: "Dr. Wilson, speaking of the incongruous associations made by Dr. Cullen, in the class Neuroses, may well ask, Where is the similarity between hydrophobia and diabetes?"

While admitting that Cullen was unfortunate in the nomenclature which he employed, nevertheless his views regarding diabetes were far in advance of his age, and in some respects agree with those which are now held, and which Hosack overlooked in his own classification of diseases. The learned Scotch professor, in his treatise on the Practice of Physic, employed this language: "That no topical affection of the kidneys has a share in producing this disease, and that a fault in the assimilation of the fluids is rather to be blamed, I conclude from hence that even the solid food taken in increases the quantity of urine voided, at the same time with an increase of the saccharine matter above mentioned. The diabetes has been supposed to be owing to a certain state of the bile; and it is true that this disease has sometimes occurred in persons who were, at the same time, affected with diseases of the liver; but this occurrence does not often take place, and the diabetes frequently occurs separately from any affection of the liver. In twenty instances of diabetes which I have seen, there was not, in any one of them, any evident affection of the liver. As I have already said," Cullen continues, "I think it probable that in most cases the proximate cause of this disease is some fault in the assimilatory powers, or in those employed in converting alimentary matter into the proper animal fluids. This I formerly hinted to Dr. Dobson, and it has been prosecuted and published by him; but I must own that it is a theory embarrassed with some difficulties which I cannot at present well remove."

Such was the philosophy taught by the Professor of the Prac-

tice of Physic in the University of Edinburgh in 1783. Its logic is worthy of our admiration, for from imperfect scientific premises there were drawn conclusions of great practical importance. Guided by these inferences Dr. Rollo, Surgeon-General to the Royal Artillery, about the commencement of the present century, was induced to restrict the diet of diabetic patients, confining them chiefly to animal food, and forbidding the free use of vegetables affording saccharine matter.

All honor to these old writers, who, though ignorant of the phenomena of glycogenesis, believed that in the disease in question there was glucæmia, and to a certain extent hypothecated the source of the sugar, and who, with a very limited knowledge of the chemical composition of food, prescribed a form of aliment which to this day is believed to be the one best suited to relieve melituria.

It is manifestly impossible, on an occasion like the present, to allude to all of those whose labors have contributed to elucidate the subject of diabetes. It must suffice to give a brief summary of the views of a few of those physiologists who, by explaining normal and artificially produced glycogenesis, have thus thrown light upon the processes of diseases favoring the abnormal production of sugar.

As is well known, Bernard, since the year 1848, has from time to time laid his experiments and conclusions before the medical public. He has shown that the liver possessed a glycogenic function; that the sugar formed did not pass through the hepatic ducts, but was carried off by the blood through the hepatic veins, and could be thence traced to the right side of the heart and to the lungs, but there disappeared, and could not ordinarily be found in the blood of general circulation. The liver, he likewise has stated, possessed the property of forming sugar after removal from the body. The sugar is not primarily formed, but is the result, he asserted, of the metamorphosis of a material known as "amyloid matter."

Dr. Pavy, of Guy's Hospital, from his experiments concluded that hepatic sugar generation was a *post-mortem* phenomenon, and that in life the liver possessed no such function.

Dr. A. Flint, Jr., from a careful series of experiments, has shown that "during life the liver contains only the glycogenic matter,

and no sugar, because the great mass of blood which is constantly passing through this organ washes out the sugar as fast as it is formed; but after death, or when the circulation is interfered with, the transformation of glycogenic matter into sugar goes on; the sugar is not removed under these conditions, and can then be detected in the substance of the liver." This phenomenon occurs irrespective of the chemical nature of the food taken, but is the more remarkable when the aliment ingested has been of an amylaceous character.

Dr. William L. Lusk has recently shown—

"1st. That the blood of the general system, in carnivorous animals confined to a nitrogenous diet, contains appreciable quantities of glucose, not only during the period of digestion, as admitted by Bernard, but even in cases where animals have been deprived of food for a considerable period of time.

"2d. That the blood of the right side of the heart contains from a quarter to half a grain of glucose per fluid ounce, under strictly physiological conditions.

"3d. That the quantity of glucose in the right side of the heart is from two to four times greater than that found under corresponding circumstances in the jugular vein.

"4th. That this excess argues a by no means insignificant amount of sugar in the pure hepatic blood, before it has become largely diluted with the comparatively non-saccharine fluids of the venæ cavæ.

"5th. That we are forced to admit the fact of sugar-formation by the liver, though we fail to detect the presence of sugar in the liver-tissue, when after death the fermentation of the glycogenic matter is prevented."

It seems to be proved by the experiments of the physiologists above cited, that the liver possesses the property of producing sugar, but the facts they have presented will not warrant us in forming a theorem to the effect that the liver is necessarily, or at least primarily, the offending viscus in diabetes. Cases of the malady occur in which we can detect no peculiar lesion in the hepatic tissue. An increased normal function of a gland, however, may not be accompanied by serious organic changes, but the result of such hyper-action may prove otherwise disastrous. Sapid substances in the mouth may excite the salivary

glands unduly, but the parotid, submaxillary, and sublingual textures may not suffer impairment; and even when the drain upon them has been prolonged, as in the case of those persons long addicted to tobacco-chewing, we may find the glands for the most part intact.

Had we no other facts to guide us than those before given, we might infer that the liver was the chief seat of mischief in polyuria, and perhaps at least take a step in advance of those writers who have placed the malady among the renal disorders. Indeed, as we may have occasion hereafter to show, the liver, in many cases, may be the substantial cause of diabetes, while in others it may not be the most essential one.

To the other physiological phenomena to which reference has been made as throwing light upon our subject, allusion will now be made.

Bernard, in 1855, called special attention to the influence of the nervous system upon the secretion of urine, etc. He found, as given by Flint, Jr., "that when irritation was applied to the floor of the fourth ventricle, in the median line, exactly in the middle of the space comprised between the origin of the pneumogastrics and the auditory nerves, the urine was increased in quantity and became strongly saccharine. When the irritation was applied a little above this point, the urine was simply increased in quantity, but contained no sugar; and when the puncture was made a little below, sugar appeared in the urine without any increase in the quantity of the secretion." These facts serve to explain many of the phenomena observed in diabetes. Bernard further showed that "irritation applied just posterior to the origin of the fifth pair of nerves is followed by a great increase in the activity of the salivary secretion."

There seems no reason to doubt that irritation applied to certain parts within the cranial cavity has an influence upon glycogenesis and upon various secretions and excretions. How many instances might also be cited to illustrate somewhat similar phenomena from *reflex* action! By exciting the salivary secretion the flow of the gastric juice can be augmented. Bernard and Pavy have shown that the inhalation of the anæsthetics and various other gases will be followed by melituria, and attribute such result to the irritation conveyed to the enceph-

alon by the pneumogastric and thence reflected. Artificial diabetes can therefore be induced by the irritation of certain nerves at their *terminal* ramifications, as well as at or near their points of *origin* within the cranium. Clinical observation of diabetes confirms the views of experimental physiologists in relation to the etiology of the malady.

And here allow me to digress for a moment from my subject in order, while referring to reflex influences upon secretion, to suggest a new theory, viz., a possible *increase of hepatic glycogenesis under certain normal conditions.*

Sugar is not ordinarily an ingredient of the blood in quantities sufficient to be mentioned in the analyses generally given of that fluid in works on physiology; and, again, this proximate principle does not normally occur in the permanent secretions and excretions. Milk, however, contains a large amount of this material: according to Robin, 37 in 1,000, and according to Pereira and Lehmann, 47 in 1,000; all these authorities agreeing that it is present in the largest proportion of any of its organic constituents.

Now, is all this saccharine matter the result of the elaboration of the mammary glands? Physiologists contend that milk, with the exception of its water and other inorganic principles, is the direct result of the action of the secreting tissues of the breast. As sugar of milk differs slightly in composition from ordinary sugar, medical authorities assert that it is prepared *de novo* in the breasts, and is not dependent on the quantity of saccharine matter in the blood, as shown by *artificially* introducing sugar into the blood-vessels of a living animal, which principle under such circumstances is excreted, it is asserted, by the kidneys.

For example, Dr. Austin Flint, Jr., in his treatise on the Physiology of Man, published in 1870 (page 81), remarks: "The lactose, or sugar of milk, the caseine, and the fatty particles, are all produced *de novo* in the gland. The peculiar kind of sugar here found does not exist anywhere else in the organism. Even when the secretion of milk is most active, different varieties of sugar, such as glucose or cane-sugar, injected into the blood-vessels of a living animal, are never eliminated by the mammary glands as they are by the kidneys; and their

presence in the blood does not influence the quantity of lactose found in the milk. All that can be said with regard to the formation of sugar of milk is, that it is produced in the mammary glands. The mechanism of its function is not understood."

Dr. F. admits, however, that "caseine is produced in the mammary glands, probably by a catalytic transformation of the albuminoid constituents of the blood." If this be true in regard to this quaternary *proteine* principle, why is the same rule not tenable in regard to *ternary* proximate principles, especially if it can be shown that during lactation there is a natural *hyperglucæmic* condition; and when we bear in mind that liver-sugar and milk-sugar are both *animal* sugars, and if not absolutely isomeric are so nearly identical in composition that a simple process of vital chemistry can convert the former into the latter?

It seems to me that the changes in the uterus and *breasts* towards the close of utero-gestation, and in the latter during *lactation*, excite the liver to increased glycogenesis by a reflex influence; and the animal sugar thus *naturally* thrown into the circulation, and which under morbid circumstances would be almost entirely eliminated by the kidneys as in diabetes, is in this instance chiefly appropriated by the mammæ, and made to subserve the important work of aiding in the nutrition of the nursing infant. Reference will hereafter be made to an instance of diabetes occurring in a woman. *Pregnancy* seemed to increase the gravity of the disorder and to induce a fatal termination of her case. (See subsequent Table of Deaths—Case 10.) This patient died on the 25th of July, 1867, in the 29th year of her age, having suffered from melituria for eighteen months. In another instance, the wife of a young physician, the diabetes was apparently induced by uterine irritation, and seemed to date from the time of conception, the patient dying after suffering from the disease three months. The breasts may possess the property, as physiologists contend, of forming sugar *de novo*, but they are very probably expressly furnished with ready-made saccharine matter in the blood, which they can either separate *per se* as lactose, or which they can convert into lactose by the mere subtraction of a few atoms of water.

It may here be of interest to cite a partial analysis of the blood drawn from a woman during lactation. In October last I inquired of Dr. Robert F. Weir, one of the attending physicians of the Nursery and Child's Hospital, if he could furnish me with a specimen of blood and of urine from a healthy nursing woman for a physiological experiment, and he politely consented to do so. On the 14th of the month my pupil, Mr. Cyrus S. Merrill, called at the Nursery, and procured from the interne two or three ounces of blood and a specimen of urine, and conveyed them directly to Dr. William T. Lusk, with a note from me requesting him to examine them for sugar; a task which, a few days before, he had consented to perform for me. Neither one of the gentlemen was acquainted with the nature of my inquiry, and Dr. Lusk was not informed that the specimens were obtained from a healthy female.

On the 15th of October Dr. Lusk wrote to me as follows: "I failed to detect the presence of sugar in the urine altogether. A scarcely perceptible trace was furnished by a fluid ounce of the serum, a quantity, however, far too small for estimation." This examination showed that the patient was not suffering with diabetes, and that the glucæmia was very slight. The sugar detected may have been a normal ingredient of the blood of any healthy individual; but is there no other aspect in which its presence therein can be viewed?

To render the examination more satisfactory it was necessary to learn the nature of the food the woman had ingested; the time between the last meal and the venesection, and whether or not the woman had nursed during the interim. On Oct. 18th Dr. W. wrote in response to my inquiries: "The woman had eaten her breakfast of bread, tea, and herrings at from 6 to 7. That the venesection was done at about 10 o'clock. That she had nursed *two* children *twice* during the time that elapsed between the breakfast and the bleeding."

From this note it appeared that the woman had not eaten unduly of amylaceous or saccharine matter to account for the glucæmia; that three hours at least had elapsed after eating, a time sufficient, under ordinary circumstances, for any slight amount of sugar from food to have disappeared; and further, that the woman having nursed two children twice had so depleted the blood of

sugar as to leave but a mere trace of what may normally in her nursing period have been contained therein, and which now was to reaccumulate. A small percentage of sugar in the blood amounts to a large quantity in the aggregate. Fibrin, according to Lehmann, occurs in the blood in the proportion of only 4.05 in 1,000, but this amount suffices to subserve important uses in the animal economy. The quantity of urea detected on analyzing the blood seems almost infinitesimal; but when this product of disassimilation is gathered by emunctories and excreted, its measure is by no means insignificant, varying, according to Parkes, from 286 grains to 688 grains *per diem.*

From these facts it is certainly within the legitimate bounds of presumption to infer that during lactation, and perhaps during a part of the period of utero-gestation, there is a glucæmic condition induced by a reflex influence as before described, and that nature has a beneficent purpose in such design and in such operation. There appears to be a nervous connection, reflex or sympathetic, between the conditions mentioned and glycogenesis.

Patients at the present day reluctantly consent to venesection as a remedial means; more difficult is it to procure the consent of a healthy nursing woman to be phlebotomized for the sake of physiological research. Dr. Isaac E. Taylor was so polite as to furnish me with two specimens of blood and of urine yielded by two robust suckling mothers in Bellevue Hospital, but Dr. Lusk found the quantities of blood after defibrination to be too small for satisfactory chemical examination. I therefore cannot, on this occasion, give any further analyses, and the few facts thus far collected have been herewith respectfully presented in order that you may draw from them your own inferences, and in the hope that they may stimulate further inquiry.

(* A few days after reading this paper before the Academy, and while still considering the subject, I met with the subjoined paragraphs, which seem to corroborate the views which have been offered in the preceding passages. The observer, to whose experiments reference will be made, has noted the *concurrence* of *lactation* and of *diabetes*, in the same manner as

* The statements contained in this parenthetical note were given at a meeting of the Academy subsequent to the one upon which this paper was read, viz., Feb. 16, 1871, the subject of diabetes being still under consideration.

others have recognized a transient melituria accompanying whooping-cough, asthma, etc. He believes the coincidence to be a normal one, and of such frequent occurrence that the detection of sugar in the urine, he contends, would almost indicate if a mother was suckling her offspring.

He has not, at least so far as I have observed, interpreted the phenomenon as has been heretofore set forth. He has remarked that saccharine urine did not occur when the breasts remained dormant, and that melituria was generally present if the mammæ performed their function, but makes no allusion to the special reflex action to which reference has been made, and does not offer a supposition that glucæmia is induced for the purpose of furnishing the breasts with sugar, the predominant organic constituent of the milk.

Durand Fardel, in his Traité du Diabète, Paris, 1869, after speaking of the experiments of Bernard, Pavy, and Lehmann, in relation to inducing glycosuria by administering undue quantities of sugar to man and to the inferior animals, and by injecting similar proportions into the veins of rabbits, states that M. Blot has observed a physiological melituria associated with the puerperal condition. The following is a synopsis of M. Blot's observations, as translated from the work of Fardel:

"1st. There exists a physiological glycosuria among all women in child-bed; among all nurses, and among about half of pregnant women.

"2d. This kind of new function is in evident connection with the lacteal secretion. It diminishes considerably in activity, rather even ceases as soon as a morbid state occurs. It reappears with the return of health and the re-establishment of lactation.

"3d. This physiological glycosuria exists equally in the cow."

If the experiments of M. Blot have been made with the accuracy which should characterize scientific investigation, there seems no room to doubt that glucæmia ordinarily accompanies lactation. For what purpose does such condition occur, if it is not for the one which has been before specified? In confirmed diabetes, sugar appears in several of the secretions; for example, Bernard has found it in the saliva, Fletcher

and Pavy in the perspiration, and the latter gentleman in the pus of an abscess. Nature seems so liberal in the production of sugar during lactation that a surplus is eliminated by the kidneys, as in morbid conditions.

To this parenthetical note I would merely add, that Simon, several years ago, observed that milk was richer in sugar during the earlier months of lactation than at later periods. This phenomenon may thus be explained: The breasts when beginning to assume activity are in a condition to induce a general disturbance of the system, and more apt to excite increased hepatic sugar formation by a reflex or sympathetic influence. When fully developed they temporarily, to a certain extent, occupy the position of glandular organs the functions of which are permanent, and consequently their tendency to excite any special or general erethism is diminished.)

In coming now to consider more minutely the subject of glycosuria, it is proper to remark that sugar is sometimes found in the urine as a mere transient ingredient. It is a well-known fact that albumen occurs in the renal secretion under various circumstances entirely unconnected with organic disease of the kidneys. We likewise detect glycosuria in occasional cases, in which the patients are not suffering from confirmed diabetes; in other words, we observe it as an ephemeral symptom, as *e. g.*, either during or after an attack of pertussis, asthma, etc.

Artificial injuries of different parts of the brain quite adjacent have different effects, as has been shown, upon the renal secretion. These effects are characterized—1st, by an excessive diuresis; 2d, by an excessive flow of water containing saccharine matter; and 3d, by the urine being normal in quantity but loaded with sugar. All these phenomena have their analogues in idiopathic diseased conditions, the first of them corresponding with diabetes insipidus; the second with the ordinary form of diabetes, and the third with cases more occasionally presenting themselves.

After deaths from melituria we may find lesions in the fourth ventricle, at a site which is the precise analogue of the one injured by Bernard in the production of artificial glycosuria in the inferior animals. Such morbid changes have been ob-

served by Ogle, by Dickinson, by Richardson, and I will hereafter take occasion to note a similar occurrence.

In this connection it is appropriate to insert the account of an autopsy as reported by Lockhart Clarke in the St. George's Hospital Reports, and as quoted by Dr. Edes: "To the naked eye the whole of the medulla oblongata appeared perfectly healthy, but on examining the sections under the microscope it was evident that the fourth ventricle, from the point of the calamus scriptorius upwards for about a quarter of an inch, was the seat of a finely granular deposit, extending for some distance down the raphe, in which, as well as in the substance of the medulla oblongata on each side of it, numerous corpora amylacea were interspersed. In no other part of the medulla, nor in the third ventricle, could I perceive any abnormal appearance. The central part of the pons seemed more vascular than usual, but this might be accidental."

If the alterations are not found in the identical locality mentioned, they may be found in close proximity thereto in parts intimately associated with it, as in the cerebral lesions of epilepsy, paralysis, chorea, etc. These latter maladies are often concurrent with polyuria, and appear to excite it; if they yield to treatment, the diabetes likewise disappears. It doubtless occasionally happens that the cerebral lesions are of secondary and not of primary occurrence.

It is a somewhat singular fact, that while melituria is such a constant symptom in the peculiar forms of nervous disease which have been mentioned, it is not a frequent accompaniment of insanity. But there are various diseases of the brain in which there may not be a concurrent polyuria; and so likewise in persons affected with lunacy, there may be no complication of disease having a lesion capable of exciting saccharine urine.

There are cases of diabetes in which autopsical examination does not reveal marked alteration in the nervous structure; the lesions detected may be remote from the cranium, and chiefly confined to the abdominal cavity, and again there may not be discovered any striking or very serious organic change either in the liver or in any of the tissues of the body; and this being the fact, we may ascribe the hyper-formation of hepatic

sugar in such an entire group of cases to simple irritation of the brain, often from external causes; in other words, the liver may not primarily be involved, but may be excited to undue activity by some mysterious reflex influence. Careful observation, however, will generally detect some lesions — such lesions being either centric or peripheral, or in both situations.

As we do not recognize any constant hepatic lesion in melituria, as we do recognize cerebral alterations in some cases, and are aware of instances of sugar formation by a reflex influence, the following definitions and classifications are suggested.

Diabetes mellitus, a disease of the nervous system depending either upon centric or upon eccentric disturbance: by centric implying cerebral lesion or irritation; by eccentric referring to peripheral lesion or irritation, by which an influence is originated and thence transmitted to the brain, and reflected either to the liver or other parts, inducing the formation of sugar, and likewise, generally, reflected to the kidneys, exciting excessive diuresis.

Diabetes insipidus, a disease of the nervous system depending either upon *centric* or upon *eccentric* disturbance; in this malady the morbid influence, whether reflected or otherwise, being chiefly directed to the kidneys.

Some such classification of the malady will enable us to distinguish the milder from the graver cases; the curable from the incurable; and qualify us to address our remedies to the removal of the causes of the disorder, and thus not fruitlessly waste exertions in temporarily dispelling some prominent symptom.

As illustrative of this point, I would state, that Dr. Robert T. Edes, in his prize essay on the "Physiology and Pathology of the Sympathetic or Ganglionic Nervous System," informs us that "Dr. Goolden has admitted every case of epilepsy, paralysis, and chorea, that has applied at his hospital (St. Thomas's, London) during his week of admission; and not only has he found sugar in a large number of cases, but he has found the sugar to disappear as the symptoms were relieved by treatment."

Can we not, in many cases of diabetes not thus induced, be

equally successful in management if we more closely study their etiology and pathology? In our present state of knowledge it is not always possible to precisely fix the original point of irritation. The liver may, in some instances, be primarily the offending viscus; even impaired function in this organ may disturb the cerebrum, which in turn by a reflex action may augment the hepatic derangement.

I would not confine the primary irritation to the liver only. Glycosuria may follow paroxysms of pertussis and asthma. Where, indeed, can we stop in searching for the initial point of departure from health? The lungs, stomach, bowels, kidneys, skin, and various other parts of the body are so intimately interlinked, and so mysteriously connected with the great nervous centre, that irritation in any of them may be instrumental in inducing the peculiar reflex action to which I have alluded. The diabetes becomes permanent for the reason that we have failed to detect and remove its cause. In many of these cases, however, the cause is doubtless irremovable, even though recognized.

As the disease we are considering is comparatively infrequently met with, we learn that the centric lesion upon which it may depend is necessarily rare, and again that peripheric irritations, though of common occurrence, seldom excite the malady. This latter statement is not surprising when we bear in mind how many individuals are exposed to other exciting causes of disease, but who escape injury. The rarity of the disorder has, however, led to the belief that its cause was ordinarily some subtile and peculiar lesion not distinctly defined.

The fact is now distinctly recognized that many cases of melituria are remediable. When the malady is dependent upon an organic centric lesion, or upon an organic peripheric change exciting sugar formation by a reflex action, the disorder may prove incurable; when, however, the disease is referable simply to a centric or peripheric irritation, the cause of irritation may be removed, and the patient relieved of the glycosuria.

We have seen that temporary or artificial diabetes may follow other conditions besides injuries to the nervous system and a subsistence on a hyper-amylaceous diet. To enlarge on this point for a moment, it may be added that Prout, Wagner, Vul-

pian, and Gallatin have detected melituria during the occurrence of boils, carbuncles, and gangrene. Pavy states that it has "been induced by impeding respiration; by poisoning with strychnia and woorali; by thrusting needles into the liver (Schiff); by chloroform and ether inhalations in warm-blooded animals; in frogs, by tying the afferent veins of the kidneys so as to increase the flow of blood through the liver (Schiff); by injecting acids into the veins." Again he says: "We ought to find saccharine urine most constantly after paroxysms of whooping-cough and spasmodic asthma, in the early stages of tetanus, after chloroform inhalation by healthy persons, soon after injuries involving the nervous centres or the liver, and in apoplectic seizures." He accounts for the absence of glycosuria in various pulmonary disorders by the fact, that when there is a pyrexial condition, or great disturbance of the general system, "the amyloid substance speedily disappears from the liver, and consequently no sugar can appear in the urine, however perfectly all the other conditions for its occurrence exist."

As impeded or imperfect respiration favors a glucæmic condition, the following thought is suggested: A considerable number of patients with diabetes perish from phthisis, or what is vaguely termed asthenia, which may include chronic pulmonary disease. In some such cases is not the diabetes induced by the reflex irritation of the tuberculosis, even in its early stage? Before attention has been specially directed to the lungs, the melituria may have attracted notice. The hectic fever finally developed does not entirely arrest the glycogenesis, for the reason that during parts of the day the patients are comparatively exempt from it. The accepted view on this subject is to the effect that phthisis is a malady ordinarily intercurrent to diabetes. While not denying this fact, I would add as a corollary to what has been before said, that phthisis may be antecedent to and the exciting cause of diabetes, irrespective of any impairment of respiration induced by tuberculosis.

Interruption to the healthy performance of respiration occasionally induces glucæmia and glycosuria. Is not a disturbance of the cutaneous function followed at times by the same sequelæ? The concurrence of melituria and furuncles, carbuncles, and gangrenous conditions, has been repeatedly noted. Does

the same morbid condition which begets these phlegmons, also engender melituria? Such may be the case, but the coincidence may possibly otherwise be explained. The skin covering inflamed centres and parts adjacent is not fitted to perform its function, and associated with the malaise there may be general cuticular irritation. The blood ordinarily loses most of its sugar in passing through the lungs, and the quantity normally remaining, if not otherwise appropriated, may possibly be eliminated through the skin as water and carbonic acid. If the cutaneous function is disturbed, such elimination may be prevented, and melituria occur as a sequel of such accumulation of saccharine material in the circulation. If there is intense inflammatory fever, this phenomenon may not occur, as during pyrexial periods glycogenesis is apparently diminished.

It may be found that in quite a number of cutaneous affections sugar occurs in the urine from a similar cause. The quantity of urine voided need not necessarily be increased. The future may show that melituria may be present under as many morbid circumstances as albuminuria. Albumen has been sought for and repeatedly found: sugar is less easily detected; its occurrence in the renal secretion has not been anticipated, and the search for it has not been sufficiently thorough, excepting when examining cases of suspected diabetes.

There certainly appears to be a peculiar relationship between the morbid condition favoring carbuncles and boils, and that inducing melituria. The former condition is accompanied with transient diabetes, the latter with cuticular phlegmons. Dr. Fouseea, of Pernambuco, states that anthraces may generally be regarded as diagnostic of diabetes. According to this writer, intercurrent carbuncles are different, however, from ordinary anthraces, in that they have smaller apices, with everted edges, and have an internal cavity like a cyst; the pus contains sugar, is freely produced, has a chocolate color, and has the odor of honey.

As preliminary to a further exposition of the subject, it is proper to remark that some physiologists contend that sugar may be formed by other structures than those of the liver, for it is stated that a glycogenic principle can be detected in the muscles and lungs of the fœtus, in the muscles of hybernating

animals, and in "limbs paralyzed by division of the motor nerves."

While not advancing the opinion that the liver is the only sugar-house of the animal economy, it must be apparent from what has been said, that the phenomena just cited can be satisfactorily explained without seeking further information than we now possess concerning glycogenesis. In the fœtus respiration has never taken place; after birth, when this function is established, the glycogenic principle disappears from the muscles and lungs; in hybernating animals respiration is to a great extent suspended; in limbs impaired by bisection of nerves, nutrition is imperfectly performed, and in the play of chemical affinities sugar formation may occur as a morbid process.

The following practical inference may be derived from this last fact, viz., that parts of the body may become enervated and produce a principle which in health they would not produce; and if the parts so involved are extensive, the blood of the general circulation may become charged with sugar, and the disease, the etiology of which we are considering, may be thus developed.

By some it has been contended that diabetes is attributable to an error in assimilation, or to disease in the chylopoietic or assistant chylopoietic viscera. With the information we now possess we can otherwise interpret the phenomena of melituria. Several autopsies will hereafter be cited in which there were no serious centric lesions, but in which were found disease of the stomach, intestines, liver, kidneys, etc. In such cases we can still attribute the disease to a nervous influence, for the morbid conditions of the parts mentioned may so irritate the nervous centre as to excite sugar formation by the reflex action to which allusion has already so fully been made.

There is, doubtless, a group of cases of glycosuria dependent chiefly upon an error of diet, as *e. g.*, an ingestion of too bountiful a supply either of saccharine or of amylaceous materials, the quantity unrequired to supply the natural demand for such principles being simply eliminated. Such condition can be ameliorated by curtailing the alimentary substances of a ternary nature, and the amelioration can be effected almost as speedily as acidity of the prima via, of the blood, and of the

urine can be obviated by diet and by the employment of alkalies.

The cases presenting this phenomenon, however, are not those of confirmed diabetes; but as they can be entirely relieved by a *chemical* adjustment of the pabulum, and as permanent melituria can be palliated by similar means, it has been contended that the disease is entirely attributable to some defect of vital chemistry.

Such a view of the pathology of the malady, however, is scarcely consistent with the apparently well-established facts which show the relationship between glycosuria and nervous irritation. We cannot as yet fully interpret nervous phenomena. In some instances the liver may not be excited to increased glycogenesis by the centric or peripheric lesion; but the respiratory functions may become disturbed thereby, and by thus causing an interruption to the natural disappearance of sugar, this circumstance may be the occasion of increased glucæmia. Under peculiar nervous conditions, sugar may be morbidly formed by other than hepatic tissues; this point has already been partially alluded to, and instances given of such occurrence. Nervous excitement will abruptly change the *chemical composition* of the milk yielded by the nursing mother, and render the secretion unsuitable for food. Similar disturbance may either arrest the elaboration or modify the character of the gastric juice, while an analogous influence operating upon the kidneys may induce an elimination differing very essentially from normal urine.

It is thus seen that nervous influences can disturb the operations of vital chemistry; and while the converse of this is also true, nevertheless a weight of testimony indicates that in diabetes the nervous element of causation must have priority in the majority of the confirmed cases of the disorder.

Admit, however, that either a persistent and undue ingestion of amylaceous and saccharine materials or defective assimilative powers may induce a morbid condition leading to confirmed diabetes, and that in such cases the nervous lesions seem to be entirely of secondary occurrence; have not such *apparent* causes an insidious *vis a tergo*, and is not such force an occult nervous influence? What has occasioned the morbid craving

for ternary principles, and what has induced a vitiation in vito-chemical assimilation?

With a superior knowledge of the uses and operation of the nervous system, as compared with old Descartes, who believed the pineal gland to be the seat of the soul, the most erudite of the present day, it must be confessed, find themselves mere tyros when attempting to interpret some of the simplest phenomena of daily occurrence in healthy vital action. If incompetent to satisfactorily solve many physiological questions, they are no better qualified to solve those of a pathological nature. While certain phenomena appear to have been clearly explained, a multitude of others will remain unexplained until there is a more precise apprehension of the peculiar nature of nervous action. Should any one as yet assume to be a *dogmatic* interpreter, he will be regarded by future generations as the Descartes of our own period.

With the views now held, however, respecting nervous influences, we are justified in ascribing diabetes to the class of neurotic disorders. The future may show that such ascription is erroneous; as premises shall gradually become more accurate, conclusions *pari passu* will become more nearly exact, until medical philosophy is established on a firm logical basis; its aphorisms analogous to those of the Father of medicine, but partaking of the accuracy of the mathematical problems enunciated by Euclid, his great cotemporary. But when shall such exactness in our favorite science be attained?

In ascribing morbid conditions to subtile nervous influences, we are reminded of the language of Paget, viz.: "While we are ignorant of the nature of some of these forces, it is better that their places in our minds should be occupied by reasonable hypotheses than that they should be left blank, or be overspread with the tinge of one exaggerated theory."

It now becomes necessary to attempt to illustrate practically what has been said preliminarily and to a certain extent theoretically, and in doing so I shall briefly analyze the histories of twenty-six cases of diabetes. Several of these cases have been under my immediate care in private practice; the majority of them have occurred in the New York Hospital, and their histories have been obtained from clinical records, some having

been under my charge, others under that of my colleagues or predecessors in the Infirmary.

The cases are arranged according to the ages of the patients.

TABLE OF TWENTY-SIX CASES OF DIABETES MELLITUS.

Number of Case.	Age.	Occupation.	Nativity.	Partial History and Possible Cause of Diabetes.	Duration before coming into Hospital or under observation.	Result.	Remarks.
1	22	Seaman.....	Mass..	Eight months before admission to the Hospital had an attack of acute nephritis.....	Recognized when placed under treatment.....	Improved...	Urine both albuminous and saccharine.
2	23	Stoker.....	Ohio..	Healthy until three months before, when he had an attack of pneumonia.	Three months....	Died.....	Gradual prostration.
3	24	Seaman.....	U. S..	Three weeks before admission complained of great exhaustion and excessive diuresis....	Three weeks.....	In statu quo.	Left shortly after coming under treatment.
4	25	Drover	N. J..	Nine weeks before suffered with dyspnœa and dyspepsia; intemperate for the last year...	Four weeks.....	Relieved....	....
5	28	Coachmaker	N. J..	Developed suddenly during an attack of partial hemiplegia.	Nine months.....	Relieved...	Urine reduced in quantity to 4¼ pints, and contained little sugar; gained in weight 15 lbs. in two months.
6	28	Carpenter...	.....	Two years before had ascites—recovered; subsequently general dropsy—chronic diarrhœa	.....	Died	At time of death had jaundice; urine suppressed; died comatose.
7	29	Seaman.....	N. Y..	Came on suddenly, with pain over kidneys and feebleness of gait.....	Three months and a half.....	In statu quo.	Eloped from hospital; died about a week subsequently.
8	29	Carpenter...	Mass..	Four years before had typhoid pneumonia, which confined him to the house eleven weeks; while convalescing diabetes showed itself.....	Four years.....	Relieved....	Had cataract; diabetes somewhat relieved; phthisis developing.
9	30	.....	N. Y..	Has always suffered with micturition; twelve years ago had pneumonia....	One year.....	Died.....	Died comatose; Bri't's disease.

TABLE OF TWENTY-SIX CASES OF DIABETES MELLITUS—(Continued.)

Number of Case.	Age.	Occupation.	Nativity.	Partial History and Possible Cause of Diabetes.	Duration before coming into Hospital or under observation.	Result.	Remarks.
10	31	Shoemaker..	Mass..	Addicted to the excessive use of tobacco.....	One year.....	Improved...	....
11	32	Laborer.....	Irel'd.	Lumbago.....	.....	Relieved....	Recorded as cured.
12	32	Seaman.....	N. Y..	Intemperate habits; has had jaundice..	Six months.....	Relieved....	Recorded as cured.
13	35	.....	Eng..	Mental anxiety concerning business; has phthisis, with cavity in left lung.	One year.....	In statu quo.	....
14	36	Seaman.....	Penn..	Year ago had remittent fever, followed by diarrhœa, which has continued most of the time since..	One month.....	Died.....	Gradual exhaustion.
15	37	Hatter.....	N. Y..	Has suffered five months with diarrhœa contracted in Mexico	While under treatment for diarrhœa.....	Relieved....	Under treatment four months and a half; gained 30 lbs.
16	38	.....	.....	States that he took cold two years and a half ago, and that excessive diuresis commenced that night.	Two years and six months.....	Relieved....	....
17	39	R. R. master	Eng ..	Has enjoyed good health until attacked with intermittent fever; about 7 months ago, after a severe day's ride on horseback, he became very stiff, and micturition commenced shortly after	Seven months....	In statu quo.	Phthisis developed.
18	40	Laborer.....	Irel'd.	No cause assigned; manifested during apparent health...	Three years.....	Relieved....	Urine albuminous and saccharine.
19	44	Moulder....	Irel'd.	While convalescing from remittent fever, attacked with lumbago and diuresis.....	Eight months....	Relieved....	Recorded as cured on register of hospital.
20	45	Seaman.....	Scot..	Came on suddenly while in apparent good health.....	One year.....	Died..... ..	Phthisis.
21	47	Man'fact'rer	Mass..	Dyspeptic for two or three years; subject to bronchitis; has been very amorous since boyhood	Recognized when under treatment for nervousness.	Cured	....
22	55	Physician...	U. S..	Active mental work; has suffered for many years from chronic diarrhœa..	Several years....	Died.....	Comatose.

TABLE OF TWENTY-SIX CASES OF DIABETES MELLITUS—(Continued.)

Number of Case.	Age.	Occupation.	Nativity.	Partial History and Possible Cause of Diabetes.	Duration before coming into Hospital or under observation.	Result.	Remarks.
23	58	Agricultu'ist	Conn.	Brother of case last cited; in early life intemperate; has suffered with dyspepsia; has numbness of head and cramp in one limb.	One year.....	Improved...	....
24	65	Teacher	Mass..	Followed a slight attack of hemiplegia; had previously taught 12 hours a day for ten years; habits sedentary...	Five years.....	Improved...	Relief afforded by diet, alkalies, and removal from city to the country.
25	66	Merchant...	N. Y..	Mental tension in business	Five years.....	Died.....	When absent from business and travelling and using alkaline waters, &c., much relieved; died from gangrene of foot.
26	75	Lawyer.....	N. Y..	Habits regular; health apparently good previously...	Six or seven years	Relieved....	....

From the above table of cases may be drawn the following summary. The average age of the patients was 37 years: the youngest, 22 years; the oldest, 75. Both of these were somewhat relieved by treatment. All were males.

As respects the nativities, 18 were born in the United States; 3 in Ireland; 2 in England; 1 in Scotland; not stated, 2. Regarding the occupations, there were: Sailors, 6; laborers and carpenters, each 2; and shoemaker, coachmaker, teacher, moulder, hatter, lawyer, stoker, railroad master, drover, manufacturer, merchant, physician, and agriculturist, of each 1; not stated, 3.

For the causes assigned, I would refer more particularly to the column in the table devoted to that subject; but they may be imperfectly epitomized as follows :—

Pneumonia	2	Dyspepsia and Intemp	1
Hemiplegia	2	" " Bronchitis	1
Lumbago	2	" " Nervous Phen.	1
Ch. Diarrhœa	2	Jaundice and Intemperance	1
Rem. Fev. and Ch. Diarrhœa	1	Excessive use of Tobacco	1
Remittent Fever	1	Mental Anxiety and Phthisis	1
Ascites and Ch. Diarrhœa	1	Mental Tension	1
Acute Nephritis	1	Taking cold	1
Renal Dis., Ch	1	Over-exertion in riding	1
Exhaustion	1	Not stated	3

Of the *duration* of the disease prior to these cases coming under annotation: in twenty cases the average length of time was about one year and eight months; in four instances the patients were unaware of the precise period they had suffered with the disease; in one case the diabetes manifested itself while the patient was under the treatment for chronic diarrhœa; and in one instance the malady was detected on the patient applying for relief from other difficulties. In the twenty cases above cited, the shortest period was three weeks, and the longest between six and seven years.

The result of these twenty-six cases was as follows; relieved, fifteen—several of these are recorded as cured; died, seven; remaining in *statu quo*, four. Of those only partially relieved, it is safe to assert that some were rendered so comfortable that they had simply to conform to the old law laid down about the year 1097, in the "Code of Health of the School of Salernum," as translated by our distinguished Fellow Dr. John Ordronaux, viz.:—

> "At least six times in every fleeting day
> Some tribute to the renal function pay."

The means of relief resorted to in the management of these patients have varied, but have been chiefly:—meat diet, bran-cakes (Camplin), baths, opium, bark, preparations of iron, kreasote, cod-liver oil, benzoic acid, alkaline salts, mineral waters, aloes, nux vomica, rest from mental labor, travelling, etc., etc.

The data herewith presented are valuable in several particulars, but are not sufficiently extended to show that either age, sex, or occupation predispose to the disease. In a subsequent

table these points will be illustrated in a more extended and satisfactory manner.

In relation to the hereditary transmissibility of diabetes, comparatively few observations have been made. Fardel, Bourchardat, Blumenbach, Isenflamm, Prout, Pavy, Alquié, Mosler, and others have reported instances in which the disorder has descended from parent to offsprings. The latter gentleman was consulted by a peasant aged forty-seven years, suffering with the disease, whose parents had both had the malady, and in the course of three months her own son, aged fifteen years, fell a victim to the complaint. The only fact enumerated in the above table which at all has a bearing upon this point relates to Cases 22 and 23 respectively.

The former of these, a prominent physician of New England, I was requested to see in February, 1869, while he was making a brief sojourn in this city. Fatigued by travel, and believing his disorder to be of a mortal character, he was depressed in spirits, and anticipated an early dissolution. He rallied from this condition, however, and returned to his home, where, until the fall of 1869, he was enabled to partially prosecute his professional and public duties. He died on the 26th of March, 1870. A brother of this gentleman is at present suffering from the same disorder. They do not appear to have derived from their parents any tendency to it; possibly a predisposition has descended from an antecedent generation, as may occur with transmitted diatheses.

In examining the causes to which the diabetes was attributed in the cases which have been tabulated, it is noteworthy that there is scarcely one of them which can be regarded as an essential one. Large numbers of persons are constantly exposed to the same conditions without having polyuria as a sequel, and we learn the important fact that diverse irritations may induce the disorder. If the malady was always dependent upon centric lesions, such as were found in the five cases carefully described by Dr. William H. Dickinson, of London (*Lancet*, Feb. 19, 1870), I feel confident that such alterations in the nervous structure would be manifested by other phenomena than are usually manifested in melituria. Such lesions, without doubt, are occasionally the primary cause of diabetes; but they are not essential to its outbreak, and may be of secondary occurrence.

It will be of interest in this connection to quote *in extenso* the views of Dr. Dickinson in relation to the pathology of diabetes, as drawn from the five cases just alluded to. He says: "Peculiar morbid changes were constantly found in the cerebro-spinal system. In all the alterations were of the same nature, and, for the most part, in the same situations. The earliest alteration recognized was a dilatation of the arteries. This was followed by a degeneration of the nervous matter at certain points external to them. An extension of the degenerative process occasioned destruction and excavation of the tissue around the vessel. Cavities were thus produced (often large enough to be striking objects even without the microscope) which contained blood-vessels, extravasated blood, grains of pigment, and the products of nervous decay. Finally, the contents appeared to become absorbed, so that simple vacuities were left.

"As to their situations, the changes occurred in constant association with arteries. They were found in every part of the spinal cord and encephalon, attaining their greatest development in the medulla oblongata and pons Varolii. The excavations were frequently in connection with folds of pia mater, and were often disposed with lateral symmetry. Several spots were found to be affected uniformly in all the cases examined—namely, the olivary bodies, the vicinity of the median plane of the medulla, the gray matter of the floor of the fourth ventricle, and, in particular, a spot just internal to the origin of the facial nerve. This point presented a remarkable sameness in all the cases, a large excavation lying generally on both sides, in connection with a process of pia mater. Another spot, which in four of the five cases presented a considerable cavity, was found near the front of the pons, in the median line, also in connection with the pia mater. The optic thalami and corpora striata were involved to a slight extent. The septum of the ventricles and the white matter of the convolutions displayed the alterations in a remarkable manner, as also did the corpus dentatum of the cerebellum. The changes especially affected the white matter, though the gray matter at the floor of the fourth ventricle and of the spinal cord are exceptions to this statement. The cells of the gray matter were generally perfect. The deterioration was not general, but was limited to the vicinity of the arteries.

Such parts of the sympathetic system as were examined—namely, the upper cervical and semi-lunar ganglia, were apparently natural. The only constant change found in the viscera was epithelial accumulation in the liver and kidneys." (*Lancet*, London, Feb. 19th, 1870, p. 269.)

Among the causes recognized as giving rise to modifications in the circulation of the nervous centres, Dr. D. especially mentions mental disturbances, rage, grief, anxiety, and toil, and the various forms of dissipation, among which sexual excess takes a prominent place.

Any observations of the eminent medical investigator from whose writings I have just quoted, are worthy of the highest consideration. The lesions which he has found, and which he has so minutely delineated, are doubtless in a number of instances of *primary* occurrence, while in others they may be of *secondary* incidence; but whether they are regarded as *prodromata* or *sequelæ*, they are not observed in a proportion of cases sufficiently large to be regarded as pathognomonic of diabetes. The causes assigned as disturbing the equilibrium of the circulation in the cerebro-spinal system are often also operative in the production of other morbid conditions, some of which may induce melituria by a reflex influence.

It might be contended that some of the cases specially alluded to in the table last given were cases of transient diabetes, and in which no inter-cranial or spinal lesions were to be expected; but in several of the cases which terminated fatally there was an absence of symptoms indicative of organic nervous disease, and autopsical examination proved that none such had existed. We are instructed by these latter cases that our attention is not to be riveted upon centric lesions only; but that in the management of melituria it is essential to scrutinize each organ and each function of the body, and if possible allay either any local cause of glycogenesis, or any irritation capable of exciting reflex sugar formation.

The group of cases before given illustrates various interesting points, but to many of them time forbids me to allude. Of one, however, I will make a passing note.

The urine voided by diabetic patients, as is well known, is generally of high specific gravity, and is large in quantity. In

one instance the invalid, who was also suffering from chronic diarrhœa, passed at most but six pints, and its specific gravity varied from 1005 to 1009. It was tested on several occasions: "Sugar was detected in the first three trials; by the fourth a substance resembling gum." That there was no mistake in the analysis may be inferred from the fact that the examination was made by Prof. Reid. This patient gradually failed, and died twenty days after his admission to the hospital. The post-mortem appearances I append substantially in the phraseology of the interne. The case occurred before my connection with the institution.

"Great emaciation, tissues dry.

"Head natural.

"Lungs offered some old adhesions, and a few small masses of pneumonia in the second stage.

"Heart exceedingly small, not larger than that of a child of twelve years, but healthy in structure and containing very little blood.

"The stomach and intestines presented throughout evident traces of former chronic inflammation; the coats, especially the mucous, were universally thickened. Near the pylorus irregular elevations of the mucous membrane existed, mostly of oblong, slightly tortuous, and rounded shape, like enlarged and consolidated veins, or globular like pears (mammillation).

"The jejunum was thickly studded with miliary bodies, semi-transparent and of moderate firmness, situated in the mucous membrane, and apparently formed by partial thickening of the membrane; they gradually diminished in the ileum, the mucous membrane of which presented numerous black points. The large intestine presented many enlarged mucous follicles, as well as a blackening of its inner coat.

"The mesenteric glands appeared healthy.

"The liver was small, much congested; gall-bladder distended with yellowish serum.

"Spleen small and firm.

"Kidneys small; capsule more adherent than usual; cicatrix of an old abscess in the cortical substance; considerable congestion, and more firm than usual."

In another case of diabetes the quantity of urine voided

varied from two to twelve quarts, the amount discharged varying with the prevalence or subsidence of a diarrhœal affection from which he had long suffered. On being examined when the quantity discharged was eight quarts, there was a marked amount of sugar, and the specific gravity was 1010.. This patient gradually improved, and on leaving the hospital was passing from two to six pints of urine *per diem*, of a specific gravity of 1014, and had gained thirty pounds in weight.

In this connection it may prove interesting to give the account of two additional autopsies; neither of them did I witness, but will quote them from the hospital records. The second case was under my care during a portion of the time he was in the infirmary.

Case 1.—The patient had suffered with ascites two years before, but from this had recovered. The urine voided varied in quantity from five to fourteen pints; specific gravity 1042.

Autopsy.—" Brain healthy.

" Both lungs splenified, the base of the right strongly adherent to the diaphragm.

" Stomach presented traces of former chronic inflammation; the cicatrices of ulcers along small curvature; a slate-colored hue about the pylorus; thickening of mucous membrane.

" Intestines, as far as examined, healthy.

" Peyer's glands natural.

" Liver universally attached by old adhesions; enlarged, firmer than usual; first stage of cirrhosis. Hepatic artery and duct much enlarged. Bile dark color.

" Spleen partially adherent, large, and more firm than usual. Its capsule presented large and almost cartilaginous patches. Kidneys considerably enlarged, good color, healthy in structure. Bladder nearly empty."

Case 2.—This patient on admission was passing nine quarts of urine, of a specific gravity of 1035; he rapidly improved under treatment, and voided but three and one-half quarts, of specific gravity 1024. Shortly after this amelioration symptoms of Bright's disease were manifested, and he died comatose.

Autopsy.—" *Cranium.*—On removing calvarium, the dura mater was found to be considerably congested; otherwise the

membranes of the brain, and brain itself, appeared to be perfectly healthy.

"*Thorax.*—Lungs for the most part healthy; of a peculiar scarlet color, as if filled with well-aërated blood. One or two spots in left lung were hepatized. Some fresh deposit of exudation on the diaphragmatic pleura of right side. Heart large and flabby.

"*Abdomen.*—Liver healthy. Spleen normal. Kidneys large and fatty."

As diabetes is a rare affection in children, it may be interesting to insert at this time an account of post-mortem of an infant under two years of age, who had died of the malady. The case is reported by Dr. James L. Brown, in the *Am. Jour. of Obstetrics*, May, 1868, and is a case to which allusion will again be made in a subsequent table.

Dr. B. says: "A post-mortem examination of the body was made by Dr. Jacobi, who saw the child about two weeks before her death. He found the brain large and well developed, having numerous and deep convolutions. That part of the arachnoid covering the anterior two-thirds of the hemispheres was not perfectly transparent, and there was considerable injection of the dura mater, in fact, of nearly the whole encephalon. There was some thickening of the arachnoid in the fissure of Sylvius, and a few granulations having the appearance of very recent miliary tubercles. The ventricles contained very little fluid and were entirely normal. The choroid plexus contained hundreds of miliary tubercles. On first inspection the lungs presented nothing abnormal, but a closer examination discovered a number of little dots, just visible, which were evidently newly-formed tubercle. The spleen was found studded with miliary tubercles, their number and development being much greater here than in any other part of the body. Nothing abnormal was found in the liver or kidneys, except that the right kidney was somewhat larger than the left, and that the cortical substance of the left was more congested than that of the right. These were the only lesions found, and being all of recent origin and obviously secondary to the diabetes, the results of the autopsy, so far as the primary disease was concerned, were entirely negative."

It appears to me that these autopsies confirm the views which have been expressed in relation to the pathology of diabetes.

Before proceeding to another division of our subject, attention is invited for a few moments to the disorder known as diabetes insipidus. A table is herewith presented of four cases of the malady occurring in the New York Hospital :—

DIABETES INSIPIDUS.

No. of case.	Age.	Occupation.	Nativity.	Cause assigned.	Duration before admission.	Result.	Time under Treatment.
1	21 years.	Factory girl.	New York.	Exposure, while thinly clad, to inclement weather.	About 1 year.	Relieved.	2 months and a half.
2	29 "	Steward.	New York.	In August had dysentery. In Sept., after exposure to wet, attacked with diarrhœa and diuresis.	1 month.	Relieved.	2 months and 10 days.
3	36 "	Servant.	France.	While under treatment for hepatitis, seized with excessive diuresis.	While under treatment in hospital.	Cured.	2 months and 16 days.
4	38 "	Seaman.	Scotland.	Has had diarrhœa for four months, and also suffered with intermittent fever.	While under treatment.	Cured.	3 months and 20 days.

As illustrating the diuresis in these cases, it may be stated that in the second case the patient voided ten quarts of urine *per diem*, of a specific gravity of 1·001. The urine table, &c., of Case 4 may be thus condensed:—

Date.	Quantity of urine voided.	Specific gravity.	Weight of body.
May 9th.	8 quarts 8 ounces.	1002	
" 11th.	9 " 16 "	1004	123 lbs.
" 17th.	6 " 8 "	1004	128 "
" 25th.	5 " 16 "	1003	133 "
June 1st.	5 " 16 "	1003	133½ "
" 9th.	8 " 28 "	1004	146 "

In scanning the causes assigned for the malady, it is noteworthy how allied they are in character to those given for diabetes mellitus. As all the cases were relieved by treatment, and did not manifest any evidences of cerebral disease, it would appear that the disorder did not depend upon any organic cerebral lesion, although it occasionally may be due to such conditions.

It will be borne in mind that Bernard has shown that irritation of certain parts of the brain will induce excessive diuresis, but not melituria. If this be a fact, it is presumable that diabetes insipidus may be due either—1st, to centric irritation; or 2d, to peripheric irritation transmitted to the brain. In the latter case a morbid influence is reflected to the kidneys, as chiefly manifested by hyper-secretion.

If we can relieve such cases, are we not warranted in the hope that in melituria not dependent upon an organic lesion we can detect the points of irritation, either centric or peripheric, and by addressing our remedies to such points, subdue the irritation and the general disturbances to which it has given rise?

To the subject of the more *immediate causes of death* in diabetes, attention is now invited. In determining these I have not thought proper to rely on the limited experiences of a few solitary observers, but have had recourse to the records of the Metropolitan Board of Health of this city, for a period extending over three years and three months. It will be remembered that in death certificates both the primary and secondary causes of decease are required. As the deaths are reported they are systematically entered, but not classified, though a general classification is made at the close of the year. To procure both causes has incurred the necessity of inspecting the individual entries of 80,016 deaths. For aid rendered in this examination I would express indebtedness to Dr. Elisha Harris, Sanitary Superintendent, to his successor, Dr. Moreau Morris, to Mr. John Bowne, Register Clerk of the Board, and to my pupil, Mr. William Oliver Moore.

Out of this large number of deaths, viz., 80,016, but 58 were recorded from diabetes, either as a proximate or remote cause; or, as it may be otherwise stated, but one death in every 1,379. To illustrate in a fuller degree the rarity of the disorder, the data of each year may be thus tabulated:—

Year.	Deaths from Diabetes.	Ratio of Deaths from Diabetes to total Deaths.	Ratio of Deaths from Diabetes to the Population.
1867	17	1 to 1,379	1 to 58,823
1868	10	1 to 2,488	1 to 100,000
1869	22	1 to 1,143	1 to 45,454
1870 (3 months)	9	1 to 724	1 to 111,111

As an additional interesting fact, it may be stated that the fifty-eight deaths were reported by fifty-eight different physicians; in not a single instance during the time specified has a medical man reported a second case of death from the disorder in question.

Can the inaptitude of our citizens to the malady be traced to favorable meteorological influences? Our climate favors the development of Bright's disease; is it inimical to melituria?

Few reliable data have been gathered in relation to the relative frequency of the disorder in different parts of the world. Fardel states that it is of common occurrence in France, in England, in Northern and Southern Europe, in the Scandinavian territory, in Russia, Spain, Italy, and the French Colonies. It appears to be on the increase in France, but Fardel suggests that the disease may be more generally recognized at the present day than heretofore.

The disorder prevails both in tropical and in Arctic regions. Of its relative frequency, however, among the inhabitants of these widely separated regions and of intermediate localities, we are still in ignorance. Torrid heat, polar cold, and subtile atmospherical conditions may not directly induce, but may *indirectly* provoke the disorder. Facts relating to these points have yet to be gathered, and we have yet to learn whether diabetes has its chosen habitats, or whether it smites indiscriminately and alike the people of every nation.

In the subjoined table the data have been substantially copied verbatim from the official records of the Metropolitan Board of Health, and compared with the original death-certificates. In a few instances the entire material was not contained therein, and to complete the table it became necessary to call upon or write to the physicians reporting the death for the information desired. It has not been convenient to do so in every case, and several medical men were unable to recall or had never ascertained the required facts.

TABLE OF DEATHS

FROM

Diabetes mellitus in New York City, for Three Years and Three Months.

No. of cases.	Deaths in each Decade.	Age. Years.	Months.	Days.	Sex.	Condition.	Occupation.	Nativity.	Cause of Death.	Complicating or Secondary Cause of Death.	Duration of the Diabetes.
1	Under 10, 2 cases.	1	11	..	Fem.			New York	Diabetes	Exhaustion	10 weeks.
2		9	1	13	"			"	"	"	3 weeks.
3	10 to 20, 3 cases.	10	..	3	Male.			New York	"	Asthenia	3 months.
4		11	2	7	"			"	"		14 days.
5		16	7	16	Fem.			"	"	Uræmia	2 years.
6	20 to 30, 8 cases.	23	..	..				Scotland	"	Exhaustion	
7		23	..	..	Fem.	Single	Milliner	Ireland	"	"	8 months.
8		26	10	5	"	Married		New York	"		3 months.
9		28	6	8	"	"		"	"	Nervous Exhaustion	5 months.
10		28	11	16	"	"		"	"	Pregnancy	18 months.
11		29	..	..	Male.	"	Cook	New Jersey (Colored)	"	Uræmia	4 weeks.
12		29	..	..	"	"	Clerk	New York	"		6 months.
13		29	7	4	"	"	Carman	"	"		2 months.
14	30 to 40, 11 cases.	30	..	..	"	Single	Seaman	New York	"	Uræmia	1 year.
15		30	..	..	Fem.	Married	Cook	Virginia (Colored)	"	Asthenia	7 months.
16		32	..	..	"	"		Pennsylvania	"		8 months.
17		32	..	..	Male.	"	Bookbinder	Germany	"		Several years.
18		32	..	22	Fem.	Widow		New York	"	Inanition	3 years.
19		32	5	23	"	Single		"	"	Effusion of Brain	
20		34	..	..		Married		Ireland	"		3 months.
21		35	..	..	Male.			England	"		
22		35	..	..	"	Married	Minister	Russia	"	Œdema Cerebri	6 months.
23		36	..	..	"	"	Storekeeper	England	"	Acute Phthisis	7 months.
24		36	2	..	"	"		Germany	"	Exhaustion	2 years.

25		40	..	..	Male..	Single..	Clerk	Ireland	Diabetes	Dropsy	1 year.
26		40	6	..	Fem..	Married		"	"	Exhaustion	1 year 11 mos.
27		42	..	..	Male..	Single..	Clerk	New York	"	Bright's Disease	2 years 6 mos.
28		45	..	11	Fem..	Widow..	Housekeeper	"	"	Pneumonia	2 years.
29	40 to 50,	47	..	21	Male..	Widow'r	Banker	New Jersey	"	Bright's Disease	6 years.
30	10 cases.	47	6	..	"	Married	Ex. U. S. Patent Office	New York	"		6 months.
31		47	6	..	"	"	Broker	Pennsylvania	"	Carbuncles	2 years.
32		48	..	..	"	"	Watchman	Ireland	"	Pneumonia	
33		48	..	..	"	"	Laborer	Germany	"	Bright's Disease	3 months.
34		48	6	21	"	"	Hotel-keeper	"	"	Gangrene left foot; disease fourth ventricle.	3 years.
35		50	..	..	Fem..	Widow..		Canada	"		2 years.
36		50	..	..	Male..		Physician	England	"	Exhaustion from Sea-sickness	Uncertain.
37		52	..	..	"	Married	Tailor	Ireland	"		6 months.
38	50 to 60,	54	..	..	Fem..	Single..	Teacher	New York (Colored)	"	Exhaustion, and finally Suppression of Urine	1 year.
39	9 cases.	54	..	..	"	Married	Housekeeper	New Hampshire	"	Exhaustion	1 year 6 mos.
40		56	..	..	Male..	Single..		New York	"	Paralysis	2 or 3 years.
41		57	..	..	"	Married	Merchant	England	"	Meningitis	3 years.
42		57	..	..	"	"	Nurse	Ireland	"		10 months.
43		59	7	2	"			New York	"		
44		60	2	22	Fem..	Married	Housekeeper	New Jersey	"		11 years.
45		61	4	10	Male..	Single..		New York	"	Severe Chill	2 years.
46		62	8	..	Fem..	Married		Switzerland	"	Asthenia	2 years.
47	60 to 70,	63	..	..	Male..	"	Cutter	Germany	"	Phthisis Pulmonalis	Unknown.
48	9 cases.	65	..	..	"	Widow'r	Carman	New York	"	Retention of Urine	1 month.
49		66	..	..	"	Married		France	"	Asthenia	Several years.
50		66	5	29	"	"	Merchant	New York	"	Gangrene of Left Foot	5 years.
51		68	..	..	Fem..	Widow..		England	"	Bright's Disease	Several years.
52		69	..	..	Male..	Married	Merchant	Connecticut	"	" "	2 years.
53		70	10	17	Fem..	Widow..		New York	"	Old Age	1 year.
54	70 to 80,	75	..	..	"	"		New Jersey	"		2 years.
55	4 cases.	76	4	..	"	"	Housekeeper	Massachusetts	"		6 months.
56		79	..	1	Male..	Widow'r	Merchant	New Jersey	"		1 year.
57	80 to 90,	83	2	16	Fem..	Widow..		New Hampshire	"	Uræmia	6 months.
58	2 cases.	85	5	22	Male..	Widow'r	Carpenter	Pennsylvania	"	Old Age	2 years.

In viewing this table it is noteworthy that neither extreme of life is exempt from melituria. The youngest victim had attained an age of but one year and eleven months, while the oldest approached the days of a nonagenarian, having reached the ripe period of eighty-five years five months and twenty-two days.

The mortality during the several decades of life from 20 to 70 has not varied to any marked extent, but its acme occurred between 30 and 40. In reference to the disease in early life, Prout, West, and Bouchut speak of its extreme rarity at that period. Hanner has given one case of it in an infant of 12 months. Bird in his translation of Bouchut annotes the fact that Prout, out of a total of 700 cases of diabetes, had met with "but one instance of it in a child five years old, and only 12 in young persons between the ages of 8 and 20."

There seems to be no valid reason why the disease should not prevail during early childhood. It has already been stated that a glycogenic principle can be found in the tissues of the fœtus, but that ordinarily it cannot be detected after the birth of the child. Is it not probable that the malady is occasionally overlooked? In the young infant excessive diuresis may escape the attention of the nurse; and if it attract notice, the difficulty in procuring a sufficient quantity of the urine for examination may preclude the recognition of melituria. In the mean time the child is gradually dehydrated, its tissues emaciated, its food undigested, and its death may be attributed to some species of marasmus, as atrophia infantilis, tabes mesenterica, etc., unless some intercurrent acute malady has proved the immediate cause of dissolution.

I would invite special attention to this point, not believing that the disease is of very common occurrence during infancy, but that it may more frequently induce death than we are generally aware. In this city, during the year 1869, more than half the deaths, viz., 12,859, were children under 5 years of age, and of this number 10,311 had not completed their second year. About 500 of these last are recorded under the headings of inanition, debility, and atrophy. As the disorder in question is more liable to be overlooked at this tender period of life than at

others, is it not possible that more deaths occur from diabetes among children than appear by the records?

From the above table it is learned that 33 of the cases were males, and 23 were females; in two instances the sex was not stated. Of those over 20 years of age 29 were married, 8 were widows, 4 were widowers, and 8 were single.

In regard to the nativities, 36 were born in the United States (three of these were of African descent), 13 in Great Britain, 5 in Germany, 1 in Russia, 1 in France, 1 in Switzerland, and 1 in Canada.

As respects the duration of the diabetes, the shortest period was two weeks and the longest two years. The facts relating to this point may be tabulated as follows:—

2 weeks	1 case.	Under 1 year	22 cases.
3 weeks	1 case.	1 year	5 cases.
4 weeks	2 cases.	1 year 6 mos.	2 cases.
2 months	1 case.	2 years	12 cases.
2 months and a half	1 case.	2 years 6 mos.	1 case.
3 months	4 cases.	3 years	3 cases.
Between 3 and 6 mos.	7 cases.	5 years	1 case.
Between 6 and 10 mos.	5 cases.	6 years	1 case.
		11 years	1 case.
Total under 1 year	22 cases.	several years	3 cases.
		Uncertain and not stated	7 cases.
		Total	58 cases.

In forty-one of these cases the secondary causes of death are given. These may be enumerated thus:—

Exhaustion and asthenia	14 cases.
Bright's disease	5 "
Uræmia	4 "
Phthisis, pneumonia, gangrene of left foot, old age—of each two	8 "
Paralysis, meningitis, effusion of brain, œdema cerebri, pregnancy, severe chill, inanition, dropsy, carbuncles, retention of urine—of each one	10 "
Total	41 cases.

From these statistics, and from those before given relating to diabetes as it appears among our own citizens, can be derived many interesting facts appertaining to its etiology, pathology, course, duration, and termination. The latter table exhibits the disorders liable to become intercurrent and to hasten an unfavorable issue, and we are thus warned to shield our patients, so far as it is possible, from their invasion.

A final subject which remains for consideration is the management of the disorder which has this evening received our attention. It is manifestly impracticable, at this late hour, to speak of all the remedies which have either been employed or suggested in the treatment of the malady, and my remarks must be chiefly confined to the philosophical principles which apparently direct a correct plan of therapeusis.

If it is found that the melituria is dependent upon an organic centric lesion, or upon an organic peripheric lesion, we must usually deplore the fact that we are incompetent to avert a fatal termination of the case. At the same time it may be possible to palliate many unfavorable symptoms, and for a season to prolong the life of the patient. If not caused by intercranial or distal lesion, the cerebral irritation or peripheric sources of disturbance may be relieved by treatment. If the irritation is cerebral, pulmonary, hepatic, intestinal, cutaneous, nephritic, etc., modify and relieve such cause and remove the diabetic sequel. Is syphilis and are other dyscrasiæ exerting their morbid influences, our remedies are to be addressed to such diseased habits.

The alkaline waters of certain German spas have an enviable reputation in the cure of the disease; but they are by no means specifics. Alkalies appear to possess either a retarding influence upon sugar formation, or the power to cause indirectly the destruction of sugar in the animal economy. It is probable, however, that in addition to any medicinal effect of the mineral waters, the surrounding favorable hygienic influences recuperate diabetic patients in the same manner that they restore ordinary valetudinarians. It is well to remember that there is no special pool of Bethesda where the "withered" diabetic can expect certain relief. With the facilities at our command, we can either import the healing waters of distant

springs, or can artificially prepare health-giving draughts and restorative baths analogous to those procured from the remote fountains of Nature's handiwork. Employing such means, and placing our patients under favorable hygienic conditions, we can either in their own homes or in their own land aid their recovery in almost as perfect a manner as though they had exiled themselves in search of a sovereign panacea.

To enlarge for a moment on the subject of the use of alkalies, etc., I would remark that in a paper read before this Academy some time since, by Dr. Hanbury Smith, "On Mineral Waters as Therapeutic Agents," Dr. S. used the following language: "In diabetes, mineral waters are not thought of till too late; yet it is incontestable that the alkaline waters always diminish the quantity of sugar discharged the first week of their employment, and soon remove it entirely, not to return for many months, and then in less proportion. Huss and others report cases of absolute cure by the Carlsbad waters, but the patients were doubtless in the first stage." * * * "In all cases," he continues, "reported cured at German springs that I have met with, there was no change of diet from that common at the spa—no special diet was enforced."

Prof. Seegan has reported fourteen cases of diabetes which he has observed at Carlsbad. In two or three weeks the quantity of sugar eliminated was reduced in quantity, and the thirst and micturition disappeared. The urine voided was eventually free from sugar. In those cases which he had the opportunity of watching for several years, he states that though the sugar was absent from the urine for a season, it subsequently returned, but in small quantities, and the health of the individuals was not interrupted.

I am indebted to my friend, Dr. Albert H. Gallatin, for the subjoined information in relation to the chemical composition of one of the springs alluded to:—

CARLSBAD (Bohemia)—*Le Sprudel.*

Altitude 380 metres—Temperature from 30°.5 to 73° (Centigrade).

	Cubic centimetres.		Grammes.
Carbonic acid, free	210.59	Carbonate Lime	0.290
Nitrogen	0.85	" Magnesia	0.057
		" Protox. Iron	0.004
	Grammes.	Basic Phosphate Alumina	0.030
Sulphate Soda	2.154	Silica	0.151
" Potash	0.053		
Chloride of Sodium	1.256		5.299
Carbonate Soda	1.304		

In one of the cases coming under my care, the patient was very much relieved while availing himself of the hygienic and therapeutical advantages afforded by the German watering-places; but on returning to this country, it was found that his disease had not been eradicated. He died a few months subsequently from exhaustion, specially induced by gangrene of the foot, the gangrene having been excited by a slight and accidental puncture of the great toe.* A species of senile gangrene of the extremities, however, is one of the terminations of diabetes, irrespective of any traumatic cause.

From the facts which have been presented, there seems to be no room to doubt that the mineral waters alluded to, and other alkaline preparations, possess a controlling influence upon diabetes. As these are by no means specifics, several questions of interest arise in this connection: in the first place, to what cases are they suited? and in the second place, in what manner do they prove remedial?

If the disease is manifestly due to organic lesions, either centric or peripheric, we can scarcely expect to effect a radical cure by the medicinal agents referred to. Nature, however, is occasionally tolerant of organic changes in important viscera; this phenomenon is particularly noticeable in the disorder which may be denominated the latent form of Bright's disease. So also where there are other structural changes; while the tissues may not be restored to their normal condition, Nature can to a certain extent accommodate herself to the abnormal condition, if the patient is placed under otherwise favorable circumstances.

* Vide *New York Medical Journal*, July, 1870, p. 566–67.

It is in certain of the cases of diabetes dependent upon centric or peripheric irritation that we may hope for a permanent cure from the remedies in question; in others, such means can only palliate.

It is difficult to determine in what manner the alkalies prove remedial. Do they prevent sugar formation? They have not the property either of oxidizing or otherwise destroying sugar, though they may indirectly cause its transformation by correcting an undue acidity of the blood. They may behave after this manner: Mineral waters applicable to chronic diseases generally act more efficiently during the warm months of spring and autumn; in other words, when their action is partially directed to the skin. Under alkaline medication, warts may disappear and various cutaneous disorders be ameliorated. It is well known that carbonic acid and water are eliminated from the cutaneous surface. If mineral waters favor a determination to the skin and favorably affect the capillary circulation, it is presumable that the sugar may be destroyed either in or near the cuticle, and be eliminated as carbonic acid and water, besides otherwise favoring diaphoresis. In health, sugar for the most part disappears from the blood in passing through the lungs; in diabetes, when patients are placed upon the use of alkalies, the surplus sugar is possibly, to a great extent, eliminated both directly and indirectly through the skin. This hypothesis is worthy of more extended consideration, and is a rationale which future observations may prove to be correct.

Time forbids any special allusion to the various other remedies which have been employed in the treatment of the disease we are considering. Several have unquestionably proved successful, but there is not a single one of them which is suited to every case of the malady. As diverse *centric* and *peripheral irritations* and *lesions* provoke the disorder, it is beyond the scope of this paper to mention the numerous tonics, nervines, alteratives, etc., suited either to palliate or to remove such conditions. The *indications of cure* have been sufficiently fully set forth, and the choice of remedies is not difficult if the idiosyncrasies of each case are closely scrutinized.

The division of the subject as has herewith been presented seems to simplify the entire method of treatment, and enables

us to use our therapeutic agents for more direct and more precise purposes than they are ordinarily employed in the management of diabetes. In this connection it merely remains to add, that by the side of the Carlsbad Spas we should group the springs of Vichy for their medicinal virtues; and again, that besides the means and the remedies spoken of in an earlier part of this paper, the inhalation of oxygen, counter-irritation, the internal administration of camphor, of bromide of potassium, of quinine, of valerian, of mercury, of arsenic, of phosphorus, of acids, etc., are doubtless of efficacy under certain conditions.

In this exposition of the subject of diabetes, the endeavor has been made to call particular attention to the causes of the disorder, and it has been done in order to show not only that such causes may occasionally be removed, but also that their sequel may be susceptible of removal. In the treatment of the malady have we not imperceptibly glided into a somewhat routine dietetic course of management, without closely scrutinizing the precise pathology of each individual case? Has not such behavior thwarted complete success? for in using mere palliative means we have lost sight of radical measures of cure.

The dietetic method of treatment has its disadvantages as well as its advantages. In the desire to preclude the use of sugar, and of ternary proximate principles liable to conversion into sugar in the animal economy, there is danger of relying too exclusively on the quaternary or protein bodies, and of inducing thereby a morbid condition tending to the exhaustion and death of the patient. This misfortune may partially be averted by the ingestion of the oils. As illustrative, in one respect, of the effects of a restricted diet, I would refer to a case in which the "temperature was found to be constantly 2° F. below that of other patients (chronic) placed in the same ward, and in other respects under similar conditions."

Prof. Haughton, in his essay on the "Application of Physics to Medicine in Modern Times," read before the British Medical Association at Oxford, in August, 1868, thus discourses on this subject, and playfully gives the English ideas of the habits of our Western brethren. He says: "The diabetic patient resembles a racing steamboat on the Mississippi whose supply of coals is exhausted, and whose cargo furnishes nothing better

than lean pork hams to throw into the furnace to maintain the race. It cannot be wondered at that our poor patient, under such disadvantageous conditions, fails to keep in the front."

The same author has thus depicted the final scene in the life of a hospital inmate: "His unfavorable symptoms (so long as his powers of digestion were not impaired) were invariably alleviated by the free use of flesh food and fat, the latter being instinctively preferred by him; so much so, that during the delirium that preceded his death for twenty-four hours, he raved incessantly about 'fat, roasted fat, which the angels of heaven were preparing for him.'"

It is not a matter of surprise that so large a proportion of patients suffering with diabetes perish from asthenia and from disorders induced by a low condition of the vital powers. There is a natural tendency in the disease to induce general exhaustion; and when to this cause of depression is added another in the deprivation of proper aliment, dissolution must be expected as a necessary sequence. A judicious employment of a dietetic method of treatment is one of the most potent adjuvants in the management of melituria; an injudicious use of it favors a termination which it is our express purpose to avert.

From what has been said it seems evident that while different cases of diabetes can, to a certain extent, be treated similarly as respects regimen and diet, there are other particulars of management which must be dissimilar, such diversities being essential in order to overcome the cause and peculiarities of each individual case.

In conclusion I would repeat, that by close scrutiny and skilful care it is possible to detect and correct various morbid irritations and conditions capable of exciting melituria. Some of these ways may even appear insignificant, and yet may be the occasion of diabetes by a reflex influence. Where the disorder is manifestly due to centric or peripheric organic lesions, it may still be possible to modify the intensity of their influences, and being aware of the intercurrent maladies most liable to cut short the life of the patient, we are warned to guard against them.

As diabetes affords few opportunities of studying its nature, it seems essential, in order that we can better understand its

peculiarities, to closely scrutinize the phenomena presented by each instance of the disease, and to note them for comparison with others.

Froude, in the introductory chapter to his extended History of England, in alluding to the past, remarks that the habits and thoughts of mankind for centuries, throughout the European world, "grew upon a single type, in which the forms of the father's thoughts were the forms of the son's, and the late descendant was occupied in treading into paths the foot-prints of his distant ancestors. So absolutely has change become the law of our present condition, that it is identified with energy and moral health. To cease to change is to lose place in the great race; and to pass away from off the earth with the same convictions which we found when we entered it, is to have missed the best object for which we now seem to exist."

It has been contended, however, that medical philosophy, though apparently progressive, often moves in a curvilinear direction, and returns in the course of time to the very point from which it received an impetus from an early investigator. On a recent occasion in this Academy it was stated by the learned Fellow who is now our honored President,* that though medical philosophy occasionally might revolve in a circle, nevertheless in its returning course it did not touch the point from which it started, but rose to a higher level, like the spiral of a screw, and with each revolution it was nearer the summit.

Cullen, in his nosology of 1785, as has been before stated, placed diabetes in the class *Neuroses;* and his followers have transplanted it hither and thither, until at the present day, in the classification adopted by the Royal College of Physicians of London, the malady appears among the *general* diseases, while in our own Board of Health it ranks among the *local* disorders.

To-night I have advocated the *restoration* of diabetes to the class of *nervous* affections, as a large proportion of the confirmed cases of melituria appear traceable to neurotic causes. Such conclusion has not been drawn from the imperfect scientific premises which guided the Scotch nosologist of the last century, but has been derived from the more precise truths of modern

* Dr. Edmund R. Peaslee.

physiology and pathology. Should such a view prevail, medical philosophy concerning diabetes will have described a circle, but in a spiral orbit, and will have risen to a more elevated position.

Many points relating to the disorder are still obscure, and the difficulties besetting their elucidation are of such magnitude that we cannot immediately surmount them. The views, however, now held respecting the disease must always be adverted to in the future, and in seeking to attain the exact truth we must hope that the geometrical curve starting from Cullen may be continued, and that in our ascent each circle may be shorter in axis, and each cycle nearer the apex of finite knowledge.

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99